Essays in Applied Labor Economics

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approved at the request of

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The Faculty of Economics, Business Administration and Information Technology of the University of Zurich hereby authorises the printing of this Doctoral Thesis, without thereby giving any opinion on the views contained therein.

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the Dean: Prof. Dr. H.P. Wehrli

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PREFACE: YOU'LL NEVER WALK ALONE

> ”I rarely end up where I intended to go,
> but often I end up somewhere that I needed to be.”
>
> Douglas Adams (1952–2001), American writer

Writing a thesis is a journey you go on your own, or at least that's what you might think. But you probably won't ever reach your destination without the constant help, advice and support from the many people around you. In fact, I think, you wouldn't even dare to start such a journey in the first place if you knew that you had to walk by yourself all the way. Fortunately though, you'll never walk alone.

First of all, I want to thank my two supervisors, Josef Zweimüller and Rafael Lalive. Josef initially awoke my interest in economics and introduced me to the fascinating field of labor economics. Josef also taught me how to frame and think about research questions and how to use empirical methods in order to find convincing answers to these questions. I also want to thank Josef for giving me this unique opportunity and encouraging me to undertake this project. Rafael taught me how to do sound and convincing empirical research and very much shaped my own way of thinking about empirical research in general, not the least because he introduced me to the world of evaluation research and thus to the essential empirical toolkit which I am using in my own research now. Thankfully, Rafael always had time to answer my many questions about economics and econometrics.

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I am also deeply grateful for the help and support from my friends and colleagues from our chair and thankful for all the discussions, either of serious or nonsensical content. Reto Föllmi and Manuel Oechslin always had an answer to practically any question. Jean–Phillipe Wüllrich actually dared to move into our office and happily turned out to be a perfect match. I apologize that he had to undergo quite some turmoils since then. Christian Hepenstrick is the only person I know who knows how to shut down the unstable root and thankfully handed me over one of the funniest "reseach" articles I've ever read (McCulloch, 1985). Claudia Bernasconi and Simone Gaillard have worked and still work at our secretary and although they didn't have to do much work for me I know they would have, if I had just asked them to. I also want to thank Simon Büchi and Bea Brunner for not shying away from the difficult research questions they were assigned to do. Besides, Simon has done an awful good job in preparing and documenting a significant part of the raw data and thus (almost) always has an answer to questions related to the data. Bea finally proved my prejudices (which I knew to be wrong in the first place, so they probably weren't really prejudices at all) to be wrong. Besides, thank you for always letting the sun shine on me!

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was taking his lectures on macro–sociology. Felix Keller gave me the unique opportunity to teach lectures on introductory statistics just after having finished my studies. I also thank the many students who took (or rather: had to take) my courses on statistics although most of them, I would guess, did not really enjoy my lectures. Nonetheless, I almost always enjoyed teaching and I hope that I could teach them some basics about the proper and sound application of statistics and econometrics.

Since this thesis is essentially an empirical project I wouldn't have got far without any data. Thus, I also want to thank the people who provided or helped with the data. First of all, I thank all the people from the "Hauptverband der Österreichischen Sozialversicherungsträger" and the "Gebietskrankenkasse Oberösterreich", who provided and helped with the data used in chapter 2. I also want to thank Alois Fässler from the Suva, who kindly provided and helped with the data used in chapter 4. And, since you don't research for nothing, many special thanks also go to the "Jubiläumsfonds" of the Austrian National Bank and the Research Funds of the University of Zürich, who thankfully provided partial funding of the research presented in this thesis.

Of course, I didn't spend all the time in my office. Thomas Scheuner introduced me to minimal music, which I love so much and which always makes me want to move. Lisa Farmer always had time to have lunch with me and charmed me with her Viennese accent. Serena Simeone took care that I never got lost in the early morning hours and always made sure that I didn't go to bed too late or too early, respectively. Sabine Regel hopefully knows that I like her because she likes me, my sometimes wacky behavior notwithstanding. Moreover, thank you for being worried about me in my risk–seeking times. Steffi Thüler and Stéphanie Zeier both went along with me for quite a significant time and although we eventually broke up, I'm glad that we shared that time. A special thank you also goes to Vreni Stadelmann, who gave me one of the most gorgeous gifts I ever got (no, I won't tell). I hope you know that I still feel very sorry for what has happend. Simone Lacher reminded me that it may still be worth taking the risk. Christian Bächle and Hannes Egli lived together with me for quite some years and thus they were both involved in my projects, whether they liked it or not. I'm also thankful for all the happy hours spent with my friends back from college: Lukas, Ruzica, Christoph, Dario, Pascal, Phillip, Claudia, Thomas and Pascale. Fortunately, we still find together from time to time.

Music is the one thing I would never want to miss and has always kept me moving, specifically in the last few nerve–racking days before finally finishing the manuscript. Special thanks to all the artists (too many to mention) for sharing their wonderful music with me and for always washing my worries away.

Finally, I want to thank my brother Thomas and his family, Sibylle, Livia and Alina for supporting me all the time. Thank you to my beautiful and beloved little niece Alina for always putting a smile on my face. Maybe you will become a scientist too, someday. Last but not least, I want to thank my parents, Dorli and Marcel, for always supporting me and believing in me. I suppose that they don't really know what I was doing all the time, which primarily means that I didn't succeed in explaining them my work properly. But still they have always been very proud of me, which in turn always made me feel very special. Thank you very much for always being there for me and for making it all possible in the first place.

Randomness has indeed a very special meaning to me by now. Of course, this partially reflects my obsession with econometric toys, where randomness indeed plays a crucial part (the little ϵ on the right–hand side of the equation). But, and I guess more importantly, this is also due to the many lucky random twists my life took in the past few years which led me to where I am now and brought me together with all these wonderful people.

Andreas Kuhn, February 2008

CHAPTER

ONE

INTRODUCTION: SWIMMING IN A SEA OF DATA...

1.1 Labor Economist Gone Astray?

"Labor economics studies how labor markets work."

George Borjas, labor economist

My thesis spans three essays in applied, that is empirical, labor economics. At least that's what I claim to do. But, as the attentive reader may have already noticed (if you didn't skip the table of contents), the topics of the three essays don't easily fit into the realm of labor economics as it is usually discussed in textbooks (see, for example, the excellent textbook treatment by Cahuc and Zylberberg, 2004). So, at the outset, I feel a little urge to argue that all three essays are *essentially* about labor economics (although all three essays also get into touch with public economics; something which though is true for most topics in labor economics). So, before moving on, let me shortly elaborate on this point.

To start with, let me clarify what probably are the most pertinent questions in labor economics in my view. I think the two topics most central in labor economics can be framed as two simple questions, since most specific questions in labor economics are somehow related to one (or both) of these questions.[1] The first question asks: Why it is that some people work and some other do not (related to this first question is of course

[1] This statement may sound a little be too simplistic – and, of course, it is. But I am talking about the very fundamental insights labor economists are striving after. To be precise, by no means do I claim that the scope of labor economics is confined to these two questions only (which I know not to be true).

the demand side of the labor market)? The second question is conditional on the first one (i.e. conditional on a worker being in the labor market in the first place) and asks: Why is it that we observe so large differences in wages, even if we sometimes think that people are not that different in the work they are doing? As I will try to point out, the three essays of my thesis all relate to either the first question (participation in the labor market) or to the second question (distribution of wages).[2]

The first essay, "The Public Health Costs of Unemployment", more or less directly relates to the question about labor market participation, although the main research question relates to a somewhat non–standard issue (from an economist's point of view), focusing on potential negative effects of involuntary loss of employment on health (more specifically health expenditures). The standard static model of labor supply is, at least from a non–economic point of view, somewhat naive with respect to this question.[3] One of the main features of the labor–supply model is, by assumption, that working (more) only induces (more) disutility, when holding income constant.[4] That is, besides from loosing part of one's income (due to the fact that unemployment benefits usually do not replace previous earnings one to one), nothing else is lost. However, there is abundant evidence suggesting that this might be a somewhat too simplistic point of view. In fact, there exists a huge number of studies about the subject, dating back at least to the famous "Marienthal" study by Maria Jahoda, Paul Lazarsfeld and Hans Zeisel (Jahoda *et al.*, 1933), suggesting that many people suffer not only from a loss of income, but may also suffer from psychological strains. In the first chapter, I thus start with the premise that people not only work in order to gain income, but also because they derive utility from working, which may be true for very different reasons. If this premise in fact is true, then the (involuntary) loss of employment should show up in a deterioration of health. In fact, economists have recently also joined in exploring the empirical link between employment and health, and these economic studies show their strength in using appropriate methods combined with good data (see, for example, two recent studies that convincingly show that loss of employment may indeed have negative

[2]The order in which the essays are presented has no deeper meaning at all, it simply reflects the chronological order in which they were finally finished. The order in which I started these projects, of course, need not have been necessarily the same.

[3]Of course, I fully appreciate the usefulness of this model (and economic models in general), which mainly derives from it's very own weakness, which is it's simplicity. And I will never argue about the fact that the model in fact yields very robust predictions regarding the behavior of individuals, which derives from its main strength, which also is it's simplicity. For a convincing example applying the model of labor supply and testing its predictions empirically, see the book by Grogger and Karoli (2005) on welfare reform in the U.S. I only want to stress at this point that we might run the risk of overlooking some important questions, if we take the models *too* serious.

[4]To be fair, economists of course know about the shortcomings of their models. Moffitt (1983), for example, is an interesting study concerned with observed deviations from the predictions of the simple labor supply model (relating to the fact that not all eligible individuals take–up welfare benefits).

effects on mortality (Gerdtham and Johannsson, 2003; Sullivan and von Wachter, 2006).

The second essay, "Subjective Evaluations of Wage Inequality and the Demand for Redistribution", clearly has a somewhat non–standard flavor too, but still it strongly relates to the central question about the distribution of wages. In a way though, I will turn the question about the shape of the wage distribution upside down. Labor economists usually focus on the forces shaping the distribution of wages. For example, much research effort has been put into exploring the effect of schooling on wages (e.g. Card, 2001), the search behavior of both workers and firms (e.g. Mortensen, 2005) or on the effect of labor market institutions (see, among many others, DiNardo *et al.*, 1996), to name but a few important examples. Now, turning the question upside down, the second essay of my thesis will take the distribution of wages as given and ask whether the distribution of wages might itself be a force of its own because individuals with different wages might behave differently (due to differences in their preferences). Such behavior may potentially (and subtly) feed back into the process of wage formation if it influences institutions which then directly impact the determination of wages (e.g. preferences over redistribution may influence the tax and transfer system via voting, which obviously feeds directly back into the labor market). This "upside–down" perspective on the distribution of wages is though common in public economics (Hindriks and Myles, 2006) and sociology (Neckerman and Torche, 2007). Again, this question departs to some extent from standard economic models in that I presume that people not only take the wage they get and live happily ever after, no matter how high a wage they actually get and independent of the other workers' wages. More specifically, chapter 3 circles around the idea that differences in wages drive differences in the demand over the amount of redistribution, which might itself feed–back into institutional arrangements via voting behavior. Institutional settings, of course, directly influence the shape of the wage distribution via their impact on the processes of wage formation. A similar story has been given be two recent theoretical studies on the relation between preferences and redistribution (Alesina and Angeletos, 2005; Bénabou and Tirole, 2006).

The third essay, "Compensating Wage Differentials and the Statistical Value of Life", also deals with wage formation. Chapter 4 is in a way the most 'standard' essay of this thesis (with regards to what labor economists usually focus on) as it deals with one of the classic ideas in labor economics (dating back to Adam Smith already, actually), which is the theory of compensating wage differentials. In economics, most arguments trying to explain differences in wages either focus on differences in individual attributes (e.g. differences in education) or institutions and legislations which also influence the wage distribution (e.g. labor market regulation). The theory of compensating wage differentials, on the other hand, states that people not only value the monetary payoff from working, but also the non–monetary amenities of their jobs (the leading example being workplace safety). And, the theory claims, firms offering "bad" jobs will have to offer higher wages ceteris paribus in order to induce workers to accept such laborious jobs. The third essay will empirically assess compensating wage differentials for non–fatal accidents at the workplace in Switzerland, and will present empirical estimates of the value of a statistical injury based on these estimates.

1.2 Data is All Around You

> "Data is not information, information is not knowledge,
> knowledge is not understanding, understanding is not wisdom."
>
> Clifford Stoll and Gary Schubert, computer scientists

In the last about ten or fifteen years, applied labor economics has been profoundly re–shaped by two parallel trends, both of which were presumably mutually reinforcing each other and led to the prevalence of empirical research in labor economics (Angrist and Krueger, 1999). First, the advance in cheap computing power and the availability of easy–to–use and fast software for statistical purposes nowadays allows us to manage and process (really) huge amounts of data. At the same time, the electronic revolution has produced huge amounts of data, some of which are available for scientific research and on which empirical research crucially rely on. The three essays of my thesis are no exception in this regard.

In chapter 2, I am working with a really unique set of administrative data, the Austrian Social Security Database (ASSD, see Kuhn and Ruf (2006) for details). These data cover the universe of private–sector workers in Austria from 1972 onwards, and they record the individual labor–market career along with information about earnings, socio–demographic characteristics and information about the employer. Moreover, these data can be linked to several other micro data sets. Specifically, I will also use data from a regional health insurance fund that essentially cover all health–related expenditures covered by the fund and include very detailed additional information about these expenditures (for example, the precise kind of medical drugs).

The third chapter relies on 'typical' survey data from Switzerland, which were collected as part of an international survey by the International Social Survey Program (ISSP). Most economists are fully aware of the potential pitfalls of survey data, most importantly missing data and measurement problems (e.g. Bertrand and Mullainathan, 2001). More often than not, they completely shy away from using such data. However, as chapter 3 will show, these data on the other hand allow the examination of fascinating research questions, which otherwise are not accessible to empirical analysis. I will also argue that the empirical analysis in this chapter tries to make a virtue out of necessity, i.e. the main focus of the analysis is explicitly based on the fact that people widely differ in the perception of the real–world.

Chapter 4 builds on a combination of administrative data and survey data (although, in this case, the survey is administered by the federal statistical office of Switzerland). With respect to the data, this chapter shows that the quality of the data can make all the difference in empirical research. First, we have access to the number of accidents within narrower groups of workers than is usually available to researchers. That is, we observe accidents not only within industries, but within cells defined over industry×skill–level of the job. Second, we can capitalize on the (partial) panel structure of the wage data, which also gives more scope for the econometric analysis.

Although, when reading textbooks on econometrics, you may get the impression that you don't really need any data for practicing econometrics, data is of course a necessary condition for doing empirical work (besides, why bother with econometrics if not because you want to analyze real–world data?). On the other hand though, the availability of data is by no means a sufficient condition for doing sensible empirical work. You also need the modern toolkit of econometrics, to which I turn next.

1.3 The Art of Drawing a Crooked Line

> "[Econometrics is] the art of drawing a crooked line
> from an unproved assumption to a foregone conclusion."
>
> Peter Kennedy, econometrician

My own approach to empirical work has been critically shaped by both theoretical and applied work on evaluation methods and statistical models for causal inference.[5] Statisticians and econometricians have long shied away from making bold statements about causality (on this issue, see the interesting and insightful epilogue in Pearl, 2000), not the least because evaluation studies have been criticized in not achieving their very own

[5]To the best of my knowledge, Morgan and Winship (2007) is the only available textbook up to date dealing exclusively with these models. However, most modern textbook treatments of (micro) econometrics discuss these methods (Cameron and Trivedi, 2005; Wooldridge, 2002) in more or less detail. The survey article by Heckman *et al.* (1999) also provides a thorough treatment of these methods. A broader and more general, and much less technical, treatment of the relevant concepts is given by Campbell *et al.* (2001).

objective (e.g. LaLonde, 1986). At this moment though, it seems fair to say that most statisticians and econometricians alike have settled on a common language, which is most often labeled the Rubin model of potential outcomes (Holland, 1986), and a very powerful toolkit in order to provide answers about the causal effects of specific treatments. This work clearly has left its marks in empirical labor economics specifically, in that applied empirical work in labor economics has partially shifted away from fully–parametric structural models to simpler methods that make the central issue of identification often much more transparent (see the insightful discussion presented in Kramarz *et al.*, 2006).

On the other hand, I am also convinced (the objection of Josh Angrist notwithstanding) that there's more to empirical labor economics than such clear–cut "what if" questions about very specific manipulations, due to the simple fact that a whole lot of interesting research questions simply do not fit into the framework of causal inference (because we can't manipulate anything we would like to manipulate). Consequently, we may forgo the chance of getting a lot of interesting insights about the social world we live in (a point of view most statisticians hold, see Gelman and Hill, 2006, for example). I am though also aware of the fact that inference in such settings must necessarily be somewhat shaky, and thus these results will always be more ambiguous. Anyway, it seems clear to me that in these non–experimental settings empirical methods are at least as important as in the typical evaluation problem setting, perhaps even more so.

The work discussed in chapter 2 is closest to the program evaluation approach mentioned before, and deals with a classical case of an endogenous regressor. Isolating the effect running from unemployment to health is primarily complicated by the fact that causation clearly can also run in the other direction, i.e. we cannot rule out on a–priori grounds that bad health is causing unemployment – rather than the other way around. What is needed here is a situation generating exogenous variation in the employment status, i.e. a situation generating variation in the employment status which is *not* driven by differences in the health status of workers. I will argue that job–loss due to the shut–down of the firm provides such a situation of a "natural experiment" (Meyer, 1995), although I will also show that there remains the problem that the bankruptcy is not random across the universe of firms. Under relatively weak assumptions, instrumental variable (IV) estimation (Angrist and Imbens, 1995; Angrist *et al.*, 1996) can be used for estimating the effect of unemployment on health.

Chapter 3 clearly departs from the approach taken in the program–evaluation literature. The research question in chapter 3 simply refutes such an approach, because the question itself cannot easily be framed in a way such that it fits into this framework. Second, even if the question *could* be framed as a causal question, the data that would be necessary simply are not available. Still, I think, using simple regression models nonetheless makes very much sense in this setting and yields interesting empirical results. Still, in this case the empirical methods are primarily used as a vehicle for describing complex and multidimensional data which otherwise would refute interpretation.

The empirical approach in chapter 4 lies somewhere between the approaches taken in chapter 2 and 3. Again, this mainly relates to the data at hand but also to the underlying conceptual problem of endogenous sorting of workers (Hwang *et al.*, 1992). Although the data do not allow for identification of the compensating wage differential via a natural–experimental situation, we can rely on the (partial) longitudinal structure of the data. This allows to use standard panel data methods, which are suited for unobserved linear fixed effects models and make it possible to empirically extract the relevant component of the observed wage (the component specific to the firm) for subsequent analysis. The results clearly show that, in this case, the combination of data and methods yield very different results.

Next, I will now discuss the three essays of my thesis in more detail, stressing specifically the main research questions and the main results.

1.4 Whereto from Here?

> "It's not what you know about something that is important,
> but rather how you use it."
>
> Adrian Pagan, econometrician

In the chapter to follow, "The Public Health Costs of Unemployment", I will explore on the relation between employment status and health. The work presented in chapter 2 studies how unemployment affects public health costs in a typical European welfare state (which is Austria). Assessing how joblessness affects health costs is difficult mainly because deteriorating health can as well be a cause rather than a consequence of joblessness. We use plant closure as an instrument for unemployment because bankruptcy is unlikely to be caused by deteriorating health but has a strong impact on workers' subsequent employment. Our empirical analysis is based on an extremely rich data set from Austria with comprehensive information on various types of health care costs and day–by–day work history of individual workers. The central findings of chapter 2 are the following. First, expenditures on medical treatments (hospitalizations and drug prescriptions) are not strongly affected by joblessness. Second, lack of employment reduces mental health for men but not for women. Third and finally, sickness benefit payments strongly increase due to job loss. Our results also show that OLS estimates strongly overestimate the causal effect of unemployment on public health costs.

The third chapter is entitled "Subjective Evaluations of Wage Inequality and the Demand for Redistribution". Chapter 3 proposes a simple framework for describing subjective evaluations of wage inequality and the demand for redistribution, and puts this framework into action using survey data from the International Social Survey Program (ISSP). The empirical analysis suggests that there is both considerable support for the equalization of wages and broad acceptance of differences in wages due to different skill levels. It is also shown that the differences in the demand for redistribution are largely due to different evaluations of what ought to be and to a lesser extent due to different perceptions of what actually is. More specifically, it is shown that the main driving source of variation in the demand for redistribution is the wish to cut wages at the top of the distribution. Further, it is shown that income on its own is a poor predictor of the desired level of redistribution, but that financial self–interest, social norms about and perceptions of issues of distributive justice all do simultaneously influence individual demand for redistribution. Finally, it is shown that demand for redistribution is a significant predictor of party preference – thus providing a potential link between beliefs about distributive justice and political outcomes.

The fourth chapter "Compensating Wage Differentials and the Value of a Statistical Injury" deals with the compensation for non–fatal accident risk in Switzerland and presents empirical estimates of the value of a statistical injury. We use data from the Swiss Wage Structure Survey (SWSS) and the Swiss Accident Insurance Fund (SAIF). The problem of main concern in chapter 4 is that there presumably is endogenous sorting of workers into jobs with different accident risks based on unobserved differences in productivity. Such kind of endogenous sorting leads to inconsistent estimates of the compensating wage differential. We approach this problem twofold. First, we have access to the number of accidents not only at the level of industries, but within cells defined over industry×skill–level of the jobs, which allows us to estimate risk compensation within groups of workers defined over the same cells. Second, we capitalize on the partial panel structure of the SWSS, which includes longitudinal information with respect to the employer. This principally allows us to empirically isolate the wage component specific to the employer. This is of central importance since the theory of compensating wage differentials essentially relates to the firm–specific component of the wage, but not necessarily to the observed wage (or the wage component specific to the worker). Our different approaches to identification in fact yield very different estimates of the value of a statistical injury. Our preferred estimate though gives an estimate of about 40,000 Swiss francs (per prevented injury per year), an estimate which actually lies within the range of estimates given by studies for the U.S. labor market. Our results also shed some light on the problem of endogenous sorting of workers.

CHAPTER

TWO

THE PUBLIC HEALTH COSTS OF UNEMPLOYMENT

Joint with Rafael Lalive and Josef Zweimüller

"We never know the worth of water 'til the well is dry." (Proverb)

2.1 Introduction

This first chapter studies the causal effect of unemployment on public expenditures on health care in a typical European welfare state. Understanding this effect is important for at least four reasons. *First*, while ill health and lack of employment are the two major risks during an individual's working life, little is known about the effects of an individual's employment status on health. In a society where all employed individuals are covered by primary health insurance, health care costs are an informative measure of the costs of health shocks to society. *Second*, understanding the causal relationship between unemployment and health care is important for both labor market policy and health policy. Labor market policies that focus on job creation might be even more beneficial to society if they are providing employment to job seekers and improving their health at the same time. Health policy makers can be interested in this relationship to assess the effects of changing conditions on the labor market on the expenditures for health care. *Third*, the effects of joblessness on public health costs may be affected by institutional rules. The public health care system of a typical European welfare state does not only cover the costs associated with medical treatment (such as doctor visits, hospitalizations, and medical drugs) but also provides insurance against income losses in case of sickness. While costs associated with medical treatment are more closely linked to a workers' health status, public health costs associated with replacement of income may be driven by institutional rules and by effects on individual incentives. *Fourth*, health care costs have risen sharply in the last decades in most industrialized countries (Hagist and Kotlikoff, 2005) and the dynamics of these costs may be related to job instability and loss of employment.

This chapter focuses on the case of Austria, which provides a good example for at least two reasons. On the one hand, health insurance in Austria is mandatory for all employed individuals (and their dependents). On the other hand, the unemployment insurance system is more restrictive than in other (European) countries and closer to the U.S. system: Regular unemployment benefits are paid for at most 30 weeks and the net replacement ratio (unemployment benefits relative to previous net earnings) is about 55 percent. As a result, job loss may have more severe financial consequences for job losers. To the extent that financial distress may lead to health problems, such effects should be more severe in the Austrian context than in other European countries.

Many public health insurance systems do not only cover costs associated with take-up of health provisions (such as doctor visits, hospitalizations, and medical drugs) but also provide insurance against income losses. While direct take–up of health provisions is more informative on the health status of an individual, public health costs associated with sickness payments are also driven by institutional rules and incentives created by such rules. Therefore, our empirical analysis will distinguish between costs associated with take–up of health provisions and costs due to sickness benefit payments.[1]

Assessing the causal effect of unemployment on public health costs is difficult primarily because deteriorating health status can be cause rather than a consequence of job loss. In other words, health selection of the unemployed will lead to a bias in the causal effect of unemployment on health costs in cross-section data.[2] To circumvent the problem of reverse causality, we use job loss due to plant closure as an instrument for individual unemployment. The experience of a plant closure strongly disrupts a worker's employment career but workers' health is unlikely to cause a plant closure.[3]

[1] When we talk about "public health costs" associated with unemployment, we strictly refer to costs that are associated with payments by the public health insurance system (sickness benefits and take–up of health provision). From the point of view of public health insurance, additional costs arise due to reduced health insurance contributions when an individual looses his or her job.

[2] Stewart (2001) shows that the more unhealthy are more likely to enter unemployment and hence the unhealthy are over-represented in the unemployment stock. Martikainen and Valkonen (1996) show that in Finland the relationship between unemployment and mortality weakened as unemployment rises, suggesting that health selection varies over the business cycle. See also the discussion on the effects of health on labor market attachment in Currie and Madrian (1999).

[3] While this chapter focuses on the impact of individual unemployment on public health costs for the same individual, a different literature looks at relationships at the more aggregate level. Ruhm's (2000) findings of lower mortality rates during recessions are consistent with such a hypothesis. Hence Ruhm (2000) documents an effect of aggregate (rather than individual) unemployment on individual health. This is in line with predictions of the economic theory of health production (Grossman, 1972)

While several other studies have adopted a similar procedure, our study has at least three innovative features that go beyond the existing economic literature on the effects of unemployment on health.

First, we study the effects of unemployment on costs associated with the take–up of primary health-care rather than on the direct effect on a workers (self–reported or diagnosis–based) health. As the Austrian system provides comprehensive coverage of health care benefits for all employed workers, the public health care system faces potentially high additional costs associated with unemployment. As a public health care system is potentially very costly to society, it is of primary interest to policy makers to have reliable information on the health costs that are causally related to workers' employment status.

Second, our study aims to give a broad picture of the overall health costs to the public health insurance associated with the experience of individual unemployment. The Austrian system does not only cover costs associated with medical treatment (such as doctor visits, drug prescriptions, and hospitalizations) but also grants sickness transfer payments both for employed workers (incapable of working due to health problems) and for unemployed workers (incapable of searching for a new job). In our empirical analysis we will assess the causal effect of unemployment on overall costs. Moreover, we also analyze the costs structure, i.e. how these overall costs are divided into the interesting subcategories.

Third, in contrast to most previous studies, we use a very large and informative data set. Our data come from the Austrian health insurance register and cover all health-care related payments to private sector employees in one large Austrian region.[4] For the period 1998–2002, we can link the health cost data with social security register data (reporting a worker's employment and earnings history). Our analysis is based on 14,602 plant–closure workers and 39,701 non-plant closure workers. One obvious advantage of these data sets is their accuracy (not prone to measurement error both with respect to health– and employment–status information). Another advantage comes from the fact that all workers have the same health insurance coverage given by a standardized catalogue of health care benefits that are covered by the public health insurance system. Hence our measure of health costs is also highly informative on the workers health status.[5]

which holds that reduced opportunity costs of time increase incentives to undertake health investments through time-consuming activities which may improve health during times of high unemployment.

[4]Our study focuses on Upper Austria which is one of totally nine Austrian states. Upper Austria, located in the North and bordering Germany and the Czech Republic, comprises roughly one sixth of the Austrian population and work force.

[5]The public health insurance system aims at a basic coverage of all major health risks. Individuals with demand for services not covered by the public health insurance system (mainly better quality, such

Our empirical analysis yields three major results. A *first* finding is that unemployment following a plant closure does not cause a significant increase in public health costs associated with take up of health provisions. Public health costs associated with hospitalizations and medical drugs prescription do not increase significantly, and doctor visits even fall. *Second*, while overall take–up is not significantly affected, we find that – for males, but not for females – mental health deteriorates. This result is in line with the hypothesis that, in the short run, unemployment causes mental health problems, whereas physical health is affected only in the long run. *Third*, we find that the public health costs that are associated with payments of sickness benefits strongly increase after a job loss. One additional day in unemployment increases the costs to public health insurance by 4.7 Euros (5.8 USD) per year for men and almost 2.5 Euros (3.2 USD) per year for women in the span of one year. However, this increase in costs does not reflect a deteriorating health status of displaced workers but is mainly due to sickness benefit rules: For employed workers, employers have to bear sickness benefits (for up to 12 weeks, depending on job tenure) whereas for unemployed workers, the public health insurance pays sickness benefits.[6] Since plant closure workers spend more time in unemployment than non-plant closure workers this increase in costs is largely mechanical. For males, we do not find that plant-closure workers do have more sickness *days* than non–plant closure workers. For females, however, we find a significant increase in sickness days.

This rest of this chapter is organized as follows. In the next section we provide a brief review of the previous literature. Section 2.3 presents the data and definitions of the crucial variables. In section 2.4 we provide some descriptive analysis. Section 2.5 discusses the econometric methodology and identification strategy. The econometric results are presented and discussed in section 2.6. Section 2.7 concludes.

2.2 Related Literature

To the best of our knowledge, this is the first study that analyzes the causal effect of individual unemployment on overall cost to public health care. This study is related to two different strands of the literature.

The first strand studies incentive effects in sickness insurance and the relationships between unemployment and sickness insurance use. Johansson and Palme (1996, 2005)

as one–bedroom hospitalization, for example) can purchase such services from private health insurance companies. Private companies cover costs beyond the public system.

[6] When a worker gets sick during an unemployment spell, the time of regular unemployment benefits is interrupted and the worker becomes eligible for sickness benefits so each day on sickness benefits prolongs the maximum duration of regular unemployment benefits.

study how changes in the income replacement level affect the incidence and duration of sick leave spells in Sweden.[7] Askildsen *et al.* (2005) argue that the negative relationship between unemployment and sickness insurance use may be due to worker moral hazard in a situation of full insurance against income loss. While our study is related to this literature, we do not assess the incentive effects of health insurance rules. This chapter contributes to this literature by studying the effects of exogenous job loss on take up of sickness benefits. Furthermore, access to direct data on health care costs allows assessing what medical conditions are prevalent among workers going on sickness insurance.

The second large strand of the literature studies the relationship between individual unemployment and individual health status. An important strand of this literature is concerned with the impact of unemployment on mortality.[8] A large number of studies have examined the effect of unemployment on mortality. Early influential studies using individual data are Moser *et al.* (1987) and Morris *et al.* (1994) who find that the unemployed have significantly higher mortality rates.[9] These studies were based on longitudinal data which control for time–invariant individual effects. However, individuals are subject to health shocks over time so health selection may be of considerable importance. More recent studies have improved upon empirical designs. Gerdtham and Johannsson (2003) use a Swedish sample of initially equally healthy individuals and show that unemployment significantly increases mortality. Using administrative data from two US states Sullivan and von Wachter (2006) estimate a 15-20% excess risk of death in the 20 years following a job loss. Eliason and Storrie (2006) show similar evidence for job losers in Sweden. Further interesting evidence comes from twins studies. Nylen *et al.* (2001) and Voss *et al.* (2004) examine mortality of Swedish twins in relation to unemployment. They find that those who were unemployment in 1973 are significantly more likely to commit suicide and or die from undetermined causes during the period 1974–1996.

Other related papers have studied the impact of unemployment on (physical and mental) health problems. Kessler *et al.* (1987, 1989) have documented the impact of unemployment (and re–employment) on self–reported health and Turner (1995) has looked at particular mechanisms (financial versus emotional distress) by which unemployment may affect health outcomes. However, it is difficult to interpret the results of

[7]See Henrekson and Persson (2004) for a related study.

[8]Cook (1985), Morris and Cook (1991) and Jin *et al.* (1995) survey the early literature. Platt (1984) documents the effects of unemployment on suicidal behavior. For recent surveys see Kasl and Jones (2000, 2006).

[9]An important strand of the literature has studied the impact of aggregate unemployment on mortality. The early work of Brenner (1979) points to a significantly positive relationship. However, the more recent literature has convincingly demonstrated that recessions and high local unemployment rates reduce rather than increase mortality (Ruhm, 2000, 2003, 2005, Gerdtham and Ruhm, 2006).

these papers as a causal impact of unemployment on health. In contrast, a recent paper by Burgard *et al.* (2005) carefully addresses the issue of health causation versus health selection in a larger sample of involuntary job losers in the U.S. While health effects are strongest for those who experience a health shock after a job loss or who loose their jobs for health reasons, adverse health effects are also existent for other workers experiencing a job loss.

The above studies are based on cross–sectional data and therefore are strongly subject to the problem of health–driven selection bias. Other studies focusing on the take–up of health care provision use empirical strategies that are less prone to such a bias. Iversen *et al.* (1989) find rising hospital admissions in a sample of Danish workers after a large shipyard closure and Keefe *et al.* (2001) report excess risk of cancer registrations and public hospital admissions in a sample of workers displaced after bankruptcy of a meat-processing plant. The recent paper by Browning *et al.* (2006) applies propensity score matching of displaced and non-displaced workers in a large administrative data set from Denmark and finds no significant effect of job loss on rates of hospitalization for stress-related diseases (such as high blood pressure, heart diseases, gastric catarrh, ulcer).

A further related literature studies the impact of unemployment on take–up of particular public health care provisions, in particular doctor visits. Carr-Hill *et al.* (1996) and Field and Briggs (2001) find that the jobless workers in the UK do consult general practitioners more often than employed workers with similar characteristics. Similar evidence was found after a large furniture plant in Austria (Studnicka *et al.*, 1991). D'Arcy and Siddique (1985) provide evidence from the Canadian health care survey data that the unemployed use public health care more heavily than workers with a job. Such evidence may indicate that unemployment leads to health problems but is also consistent with the economic theory of health production (Grossman, 1972), which predicts increased incentives to invest in time–consuming health activities during periods of reduced opportunity cost of time (such as unemployment). However, other studies find that the unemployed make less use of the public health care system even when they are eligible to health care services. Ahs and Westerling (2006) and Virtanen (1993) study Scandinavian experiences and that find that unemployment is associated with lack of unmet care needs, particularly among unemployed who suffer from psychological symptoms. One possible explanation for such a result is based upon the behavioral model of health care use (Andersen, 1995) which stresses that take–up of health care benefits is not only influenced by need of care but also by individual predisposition and social context.

Further studies have shown that unemployment has a pronounced effect on subjective well–being. Clark and Oswald (1994) and Winkelmann and Winkelmann (1998) document the close relation between unemployment and unhappiness. Theodossiou (1998) finds that the unemployed suffer more from anxiety, depression and loss of confidence compared to otherwise similar employed individuals. Bjorklund (1985) finds evidence that unemployment has detrimental health effects in Sweden. Other studies focus on youth workers and find detrimental effects of unemployment on well–being, as Goldsmith *et al.* (1996) for the United States and Korpi (1997) for Sweden.

2.3 Data and Definitions

2.3.1 Data Sources

We draw on social security register data that can be linked to data covering the take–up of health insurance provisions form the statutory health insurance fund from a large region in Austria ("Upper Austria").[10] This data set covers individuals that are employed in the private sector. Social security register data provide information, on a daily basis, on the workers' earnings and employment history (collected for the purpose of calculating a worker's old age social security benefits, see Kuhn and Ruf (2006) for details concerning this data source). Data from the statutory health insurance record all payments by the health insurance fund related to a worker's take–up of health care benefits.

The combination of these two data sets provides enormously rich information and has two additional features that make it ideally suited for the present analysis.

A *first* unique feature is that the data cover the *universe* of the private sector employees (more than 80% of the active state population) in the region.[11] Moreover, each employed worker can be linked to a particular firm via a unique firm identifier. Because the data set covers the universe of workers we can perfectly reconstruct firms. A "firm" is simply defined as the set of individuals that is observed under a given employer social security number ("firm identifier") at a given date. The possibility of linking firm- and worker–information is particularly helpful for our estimation strategy which relies on a firm characteristic: the date of shut–down of a firm. Firm information is further helpful in making plant–closure workers better comparable to employees in ongoing firms and

[10] The administration of health insurance in Austria is divided into regional units ("Gebietskrankenkassen", GBKK), and our data set comes from the insurance fund covering the region of Upper Austria ("Oberösterreich"). Upper Austria is one of the nine Austrian states and located in the northeastern part of the country. This region covers about one sixth of the total Austrian population and work force.

[11] There are separate funds for private-sector employees, self–employed, farmers, public sector workers, and employees of several public utility firms. The data available to us comprises the universe of private sector workers only.

thus allowing to compare samples of workers with similar previous job situations.

A *second* unique feature is that these two data sets provide high–quality and comprehensive information on expenditures associated with a worker's health status. The reason is that health insurance is mandatory for all employees in Austria and that coverage is comprehensive and covers all costs associated with primary health care such as treatment by physicians, drug prescriptions, and hospitalized care. As a result, the data give a very detailed and broad picture of the health expenditures caused by a given individual.[12]

The payments recorded in the data can be broadly divided into the following four categories (see table A.1 in the chapter appendix for an exact definition of these categories and further subcategories used in the empirical analysis below):

1. **Sick leave transfers:**
   These are payments to employed and unemployed workers during periods of sickness (when they are not capable of searching for a new job or not capable of working). When unemployed, sickness benefits are roughly equal to unemployment benefits. Days of sickness benefits do not reduce the number of (remaining) days during which an unemployed worker is eligible to regular unemployment benefits. When employed, a worker initially continues to receive her or his wage during the first up to 12 weeks (depending on previous tenure) in the sick leave spell. Thereafter the health insurance provides sickness benefits amounting to 80% of the previous wage. In order to claim sickness benefits, a physician has to approve and repeatedly check a worker's impaired health situation.

   Our data cover all days on sick leave but only the sickness benefits paid by health insurance. We therefore provide separate results for sickness benefits and days on sick leave. Sick leave payments may be higher for workers getting ill after a plant closure because the bankrupt firm can not continue to pay the wage for the initial 12 week period or because plant closure workers are more likely to enter sickness insurance from unemployment. Plant closure workers are thus more likely to be receiving sickness benefits paid by health insurance. The situation is different for a worker getting ill in a firm that is not going bankrupt. Thus, sickness benefits can be mechanically higher for workers in plant closure firms than for workers in

[12] On top of mandatory public health insurance, individuals purchase supplementary insurance offered by private insurance companies. Provisions provided by these companies include higher quality treatment (e.g. single bedrooms during hospitalization) and specific treatments (e.g. non–standard treatments not generally accepted by orthodox/traditional medicine). As costs covered by these supplementary insurance contracts are *on top* of provisions covered by public health insurance, this does not cause any measurement problems for our empirical analysis.

continuing firms. However, days on sick leave are recorded in the same manner for workers leaving bankrupt firms and other workers.

2. **Consultations:**
   Doctors have contracts with the public health insurance and get paid a standardized rate for each consultation.

3. **Hospitalizations:**
   The data record each hospitalization and details the particular reason for the hospitalization. In particular, it classifies the costs by the main diagnosis of the hospitalization according to the ICD–9 codes. We aggregate the diagnoses into the following causes for hospitalization: cancer, heart disease, mental health problems, respiratory diseases, cerebrovascular diseases, costs related to pregnancies, and other hospitalizations.

4. **Drug prescriptions (including detailed types of prescribed drugs):**
   The data record all payments to drug stores (or refund to individuals) for prescribed and self-medicated drugs. The data are extremely detailed concerning the type of drugs. We classify these drugs into a category that is "specific" to treat health problems associated with unemployment and a residual category of non-specific drugs. Among specific drugs we distinguish "psychosomatic" drugs (targeted at psychosomatic afflictions such as migraine therapeutics, anti-inflammatory drugs, etc.) and "psychotropic" drugs (treating psychological distress such as sedatives, benzodiazepins, antidepressants, etc.).

Table 2.1 gives an overview of health costs incurred in the year before the reference date (see the following subsection for the definition of the reference date). Overall yearly health costs of the workers covered in our sample amount to 456 Euros for men and 469 Euros for women.[13] A large fraction of these overall health costs is due to sick–leave transfer payments. For men, about 45 percent of overall health costs are due to such transfers whereas for women sickness transfer payments account for less than 20 percent. The main reason for this difference is that sick-leave benefits are closely linked to previous earnings. Hence the higher payments for men are mainly due to the fact that males get higher benefits per day on sick leave and to a lesser extent to more days on sick leave (women spend 10.4 days on sick leave, men remain 11.5 days on sick leave). Other

[13]Notice that these numbers are based on prime–age workers and are not representative for the whole population. To be included in the sample, a worker had to be employed at some date during our observations period (see below). The numbers would be much higher for retirees. Also note that the mean health costs are much lower than the standard deviation of health costs. This is due to the fact that most individuals are generating very low or zero health care costs but a small fraction of the population is incurring very large health care costs.

health costs arise from medical treatments. The remaining categories health costs caused by men amount to 81 Euros for doctor consultations, 116 Euros for hospitalizations, and 56 Euros for drug prescriptions. For women, health costs are considerably higher in each of these categories. Their yearly costs arising from doctor consultations are 156 Euros, from hospitalizations 136 Euros and from drug prescriptions 85 Euros. This descriptive analysis shows that public health costs differ strongly between women and men which suggests analyzing the effects of unemployment on health care use separately for women and men. The data on transfer payment from the health insurance fund cover the five–

Table 2.1: Health indicators, one year *before* the plant closure date

	Men	Women
Overall health costs	455.497	469.108
	(2918.971)	(1725.697)
Sick leave payments	202.469	92.008
	(2662.438)	(1314.157)
Days on sick leave	11.464	10.373
	(20.243)	(18.947)
Consultations	81.270	155.789
	(124.490)	(172.825)
Hospitalizations	115.658	136.051
	(574.983)	(574.091)
Drugs	56.100	85.260
	(305.577)	(411.819)
n	33,352	19,243

Notes: The table shows means (standard deviations) for all variables. All variables (except days on sick leave) are measured in nominal Euros and cover the four quarters before the plant closure (reference) date. See table A.1 in the chapter appendix for the definitions of the various health measures.

year period from January 1, 1998 to December 31, 2002. To get information on the the days not employed as well as on tenure with the current firm, we linked the data from the health insurance fund with the Austrian social security register data (ASSD) provided by the central social security agency ("Hauptverband der Sozialversicherungsträger").[14]

To link the information of individual health costs and individual unemployment experiences we construct a monthly panel of individuals' health and employment histories. Within the period January 1, 1998 and December 31, 2002, we measure the health costs of an individual by calculating overall health costs and disaggregate these costs into the

[14]The Central Social Security Administration gets its data from the Funds and processes this information for the purpose of calculating of old-age social security benefits. So retrospective data from the Central Social Security Administration are collected in the same way as the recent data from the Fund.

above categories (and, in the case of drug prescriptions and hospitalization costs, also into subcategories). We measure unemployment by the days not employed per month. This measure is not sensitive to individual's decisions to eligibility for unemployment benefits and actual take up decisions. The disadvantage of the days not employed measure is that there is a mechanical relationship between days not employed and days on sick leave or days spent in the hospital because these days can not be recorded as days employed.[15] Note, however, that using days not employed as a proxy for unemployment gives rise to a lower bound on the causal effect of unemployment on health rather than an upward biased effect. This is because the IV estimator relates the effect of plant closure on health to the effect of plant closure on days not employed. Using days not employed as a measure of unemployment inflates the effect of plant closure on unemployment and therefore provides a lower bound on the causal effect of unemployment on health.

2.3.2 Definition of Plant Closure

In order to assess the causal impact of unemployment on health costs we use plant closure as an instrument for an individual's unemployment. The assumption is that workers in a plant-closing firm loose their job involuntarily, whereas for other job separations it is not clear whether such separation results from a quit or a layoff. Let us first make precise how we define a "plant closure" and how we define a job-loss due to plant closure.

Definition of plant closure firms. To identify plant closure in our data it is particularly helpful that employer and employee information can be matched. In a first step, we use this information to identify, a "plant closure". A firm is considered as a plant closure firm if it fulfills the following criteria: (i) There has to be positive employment through at least 12 months up to some month t and zero employment from month $t+1$ through month $t+12$. (ii) If a firm disappears at date t, no more than 50 percent of the employees switch to the same employer at date $t+1$. (This latter criterion is adopted to rule out misclassification of a take-over as a bankruptcy). Whenever more than 50 percent of the employees are found under an identical new employer identifier these observations are excluded from the sample. To make the distinction between plant closure firms and non–plant closure firms as clean as possible all firms with large and long–lasting drops in employment (and thus all workers employed in theses firms) are excluded from the sample.

[15] There are no important differences between using days not employed or days unemployed from the perspective of eligibility for sickness benefits. Both registered unemployed job seekers and job seekers who have exhausted unemployment benefits are eligible for sickness benefits if they are eligible for unemployment assistance.

We consider all plant closures that take place between January, 1 1999 and December 31, 2001 (using the 10th of each month as the baseline date). This ensures that we have at least one year of health insurance information before and after the plant closure date (recall that the health insurance data runs from January 1, 1998 until December 31, 2002).

Definition of plant closure and non–plant closure workers. Just like plant closure firms, we define plant closure workers in a narrow sense. Our plant closure sample (PC) consists of all workers, who are employed in the month of plant closure or who were employed at least one month during the year before plant closure (but left before the effective shut–down of the plant). Hence our sample of plant–closure worker covers "stayers" who are employed in the closing firm in the month before it shuts down but it also includes "early leavers".

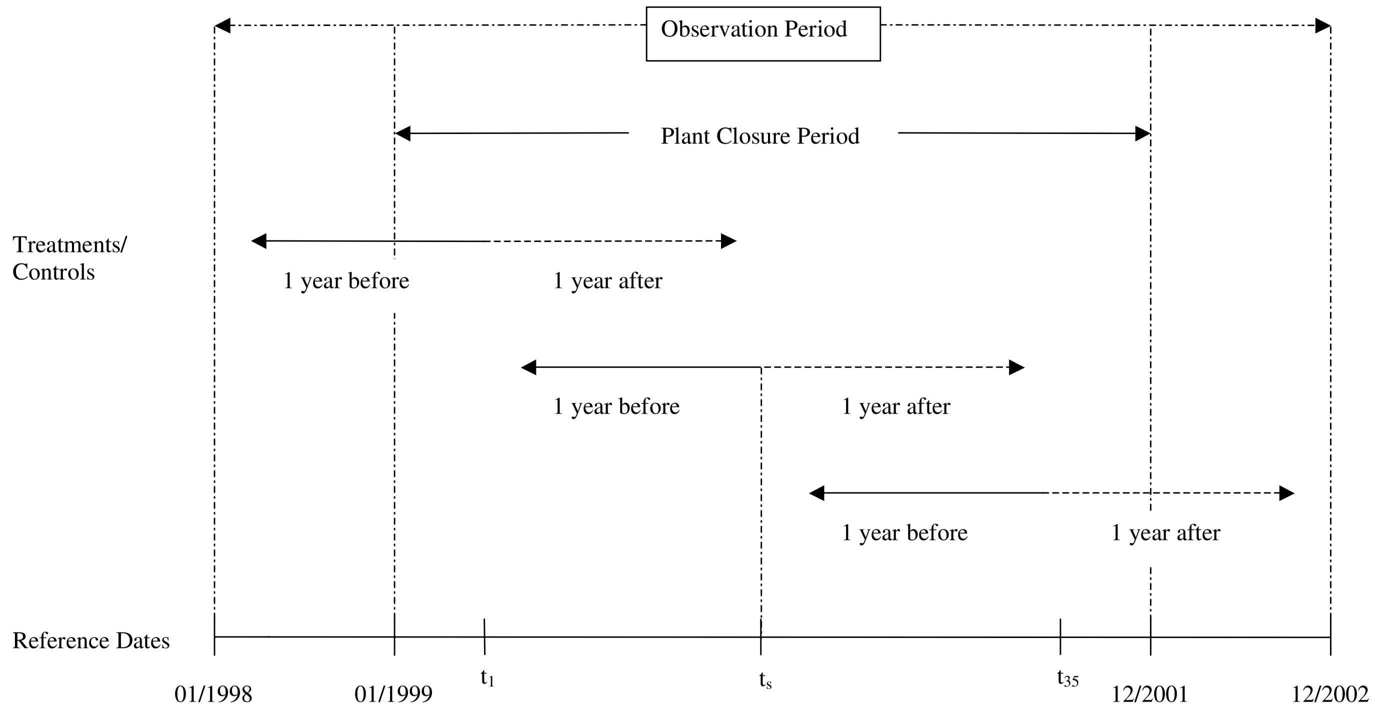
Due to the complex structure of the health insurance data, we work with a random sample of workers employed in non-plant closure firms. Non–plant closure workers are sampled randomly among all workers employed in non-plant closure firms and non-distressed firms. Specifically, on each reference date between January 10, 1999 and December 10, 2001, we take a 2.5 % random sample of from the universe of the control group of all small firms (3 or 4 employees) and a 0.25 % sample of all larger firms (more than 4 employees). All employees in firms with less than 3 employees are excluded from the data. (If such a firm disappears, it is likely that this is just a recoding of the firm identifier rather than a bankruptcy). This procedure provides a sample of control workers who were not employed in plant closure firms. Notice that the sampling procedure allows for workers to be included in the control sample repeatedly.

We measure monthly health care costs and monthly days in unemployment relative to the plant closure date for plant closure workers and relative to the reference date for non–plant closure workers. The *plant closure date* is the 10th day of the month before the plant closes for "stayers" and the 10th day of the month before leaving the firm for "early leavers".[16] The *reference date* for control workers is the 10th day of the month in which the control workers are sampled.[17] In the following, we use the term "plant closure

[16]For instance, suppose a firm is active on the 10th of January 2000 but no longer active in any of the subsequent 12 months. The plant closure date of workers who are employed in this firm on the 10th of January 2000 is the 10th of January 2000. An "early leaver" is a worker who has been employed in this firm on the 10th of February 1999 (10th of March 1999, ..., 10th of December 1999). The plant closure date for this worker is the 10th of February 1999 (10th of March 1999, ..., 10th of December 1999).

[17]For instance, suppose a control worker is included in the random sample drawn on 10th of January 2000. This individual's reference date is the 10th of January 2000. Moreover, this individual is going to be used to estimate the counterfactual for all workers employed in plants that close between the 10th of January 2000 and the 10th of February 2000.

Figure 2.1: Setup and definitions



Notes: Reference dates are the 10th of each month, there are 35 such dates in total. t_1: January 10, 1999; t_2: February 10, 1999; …; t_{35}: December 10, 2001. If a plant closure occurs between t_s and t_{s+1}, t_s is the reference date. For each plant closure date, we draw a random sample of control observations among all employed workers. Plant closure workers are workers employed at dates t_{s-4}, t_{s-3}, t_{s-2}, or t_{s-1} in the PC firm. Workers in the control sample include all workers employed at the reference date. (Notice that the same worker may be repeatedly included in the control sample, if employed at more (or all) reference dates.)

date" to identify the plant closure date for plant closure workers and the reference date for control workers.

Figure 2.1 illustrates the construction of our dataset. On each date between 10th of January 1999 and 10th of December 2001, we first identify closing firms. In a second step, we identify the workers employed in the closing firms. In the third step, we we draw a stratified (by firm size) random sample of workers who are employed in firms that do not close. The fourth and final step consists of constructing information on work history and health care costs covering the year before and the year after the plant closure or reference date.

2.4 Descriptive Analysis

It is interesting to take a first look at the characteristics of the treated (plant–closure, PC henceforth) and control (non–plant closure, NPC henceforth) groups. Applying the sample selection procedure described above leaves us with 52,595 individuals of which 12,894 are PC–workers and 39,701 are NPC–workers.

2.4.1 Ex Ante Differences in Health Costs

We first provide in–depth summary statistics on the year before the plant closure date. This provides a first test of the comparability of workers in PC–firms and workers in NPC–firms. Table 2.2 reports the health costs per individual in the year before plant closure, by gender and plant closure status. Clearly, plant closure workers incur somewhat higher health costs than non-plant closure workers. Women who work in firms that go bankrupt incur about 15 Euros in health costs more – 3% of overall NPC-costs – than women in the NPC-sample. PC–men generate 68 Euros higher – 15% of overall NPC–costs – health costs than NPC–men. Thus, female plant-closure and non-plant closure workers appear to be more comparable than their male counterparts. The differences in health costs before plant closure primarily arise because sickness benefits are higher for PC–workers than for NPC-workers (72 Euros for men, 21 Euros for women, second row)– primarily due to more days on sick leave (third row). In contrast, the remaining health cost categories are more balanced. Plant closure workers incur slightly lower costs due to doctor consultations and drug prescriptions and somewhat higher costs due to hospitalizations. This pattern is quantitatively and qualitatively similar for women and men. Thus, PC– and NPC–workers are quite similar with respect to the consultations, hospitalizations, and drug prescriptions but not with respect to sick leave. Table A.2 in chapter appendix provides more detailed information on the other

Table 2.2: Health indicators, one year *before* the reference date

	Men		Women	
	NPC	PC	NPC	PC
Overall health costs	438.205	505.810	465.721	480.660
	(2747.959)	(3367.170)	(1645.366)	(1975.412)
Sick leave pay	183.928	256.415	87.160	108.542
	(2504.585)	(3075.511)	(1276.383)	(1435.563)
Days on sick leave	11.018	12.760	9.944	11.835
	(19.872)	(21.232)	(18.705)	(19.684)
Consultations	84.978	70.481	156.622	152.946
	(128.325)	(111.903)	(174.884)	(165.596)
Hospitalizations	110.947	129.365	134.092	142.731
	(561.246)	(613.031)	(546.794)	(658.760)
Drugs	58.351	49.550	87.846	76.441
	(317.279)	(268.557)	(438.260)	(304.736)
n	24,821	8,531	14,880	4,363

Notes: All variables (except days on sick leave) are measured in nominal Euros and cover the four quarters before the plant closure (reference) date. NPC (PC) refers to the non plant closure (plant closure) workers. Also see table A.1 in the chapter appendix for the definitions of the various health measures.

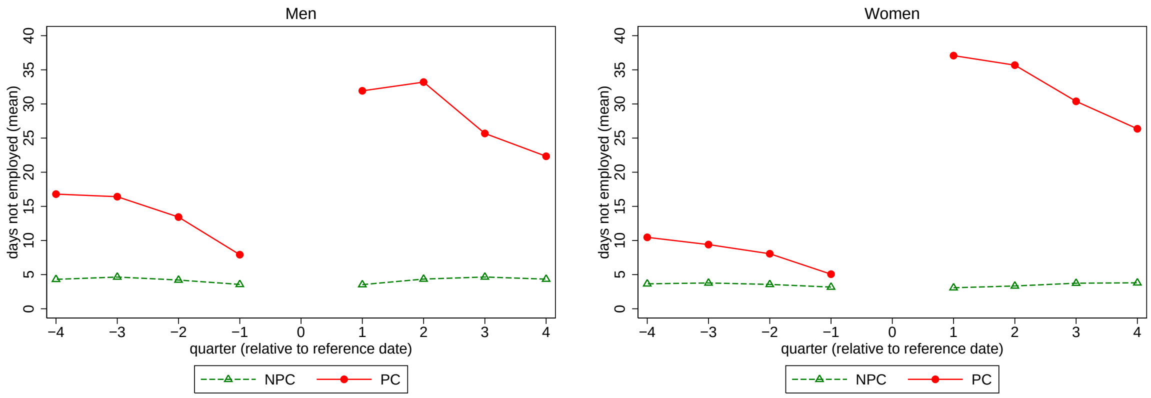
background characteristics, by gender and plant closure status. The majority of male PC–workers are blue collar workers, relatively young (on average 35.6 years) and earning a relatively low income (on average about 19,5 Euro during the year before the plant closure date). The sample of male NPC–workers has a substantially lower fraction of blue collar workers, is somewhat older (on average 36.9 years) and is earning more (on average 23,735 Euros during the year before the plant-closure). The differences between female PC– and female NPC–workers are qualitatively similar. NPC–workers were somewhat less frequently without work during the year *before* the plant closure date – on average men were 22.9 days non-employed, and women were 24.4 days non-employed. In contrast, male PC–workers spent 55.9 days in non–employment and female PC–workers were 34.0 days non-employed. NPC–workers have also been more continuously employed with their employer than PC–workers. Average tenure in the last five years is 3.2 years for men and 3.3 years for women in NPC–firms. In contrast, PC–workers joined their current employer more recently with tenure amounting to 1.8 years for men and 2.5 years for women. Table A.2 also provides information on firm characteristics, such as size, industry, and location. The data clearly show that PC-firms are much smaller than NPC–firms, and that PC–firms tend to be more likely to be in the construction sector than NPC–firms. There are no important differences with respect to firm location for those firms employing the men in our sample. In contrast, there is a higher proportion of

NPC–firms with unknown location employing women.[18] Finally, plant closures appear to be concentrated in December.

2.4.2 Effects on Non–Employment and Health

In a second step, we assess the effects of the plant closure event on the days spent in non–employment and on health care costs. Figure 2.2 depicts both the evolution of unemployment experiences for PC– and NPC–workers, by gender. The unemployment measure used in figure 2.2 is the number of days not in employment per individual during the last quarter. We see that male PC–workers spent about 15 days per quarter in unemployment throughout the year before the plant closure date.[19] In contrast, NPC–

Figure 2.2: Nonemployment, by sex



Notes: The vertical axis measures, for any given quarter since the date of (to) the plant closure (reference date), the average number of days not employed among plant closure (PC) and non plant closure (NPC) workers. For the definition of plant closure date (for PC workers) and reference date (for NPC workers) see figure 2.1.

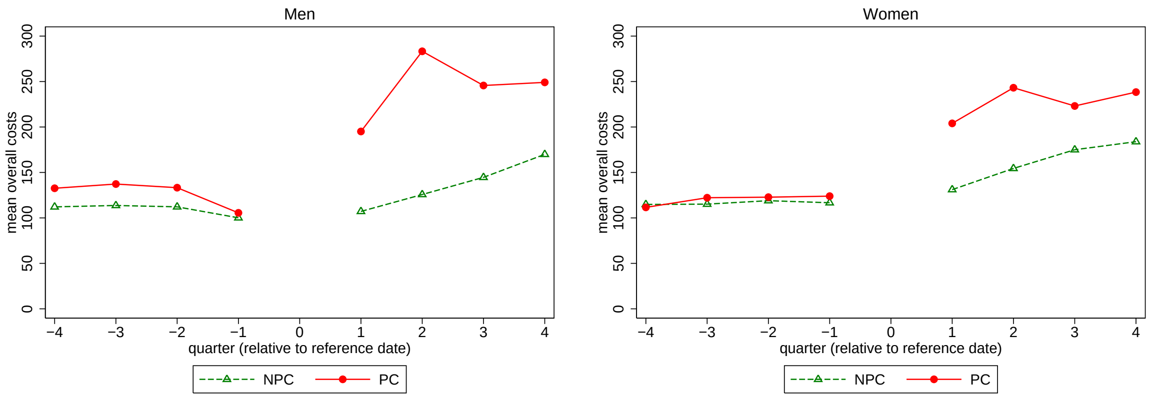
workers only spent about 5 days in unemployment per quarter during the last year before the reference date. After plant closure, unemployment soars for plant closure workers. In particular during the first six months the nonemployment rises to an average of over 30 days or more. In the third quarter after plant closure, nonemployment decreases to 25 days, and in the fourth quarter, nonemployment decreases to slightly more than 20 days – a level which is still markedly higher than in any quarter before plant closure. In contrast, unemployment for non-plant closure worker is very similar before and after the plant-closure date.

[18]Region and industry information can be missing for firms having several plants across Austria which are active in more than one industry.

[19]The difference in the number of days in nonemployment decreases somewhat before the plant closure date due the fact that each worker in the sample has to be employed on the reference date.

Women in PC–firms also spend more days in nonemployment (about 8 days) than women in NPC-firms (about 4 days) in any quarter before plant closure. In comparison with men, this difference in smaller. Yet, the impact of plant closure is even stronger for women. In the quarter after plant closure, PC–women remain in non–employment for more than 35 days whereas there is no effect for NPC–women. The discrepancy in days non–employed decreases somewhat but even in the fourth quarter after plant closure, PC-women remain nonemployed for 30 days whereas NPC–women remain nonemployed for about 4 days. Figure 2.3 shows the corresponding graphs for the evolution of health

Figure 2.3: Overall health costs, by sex



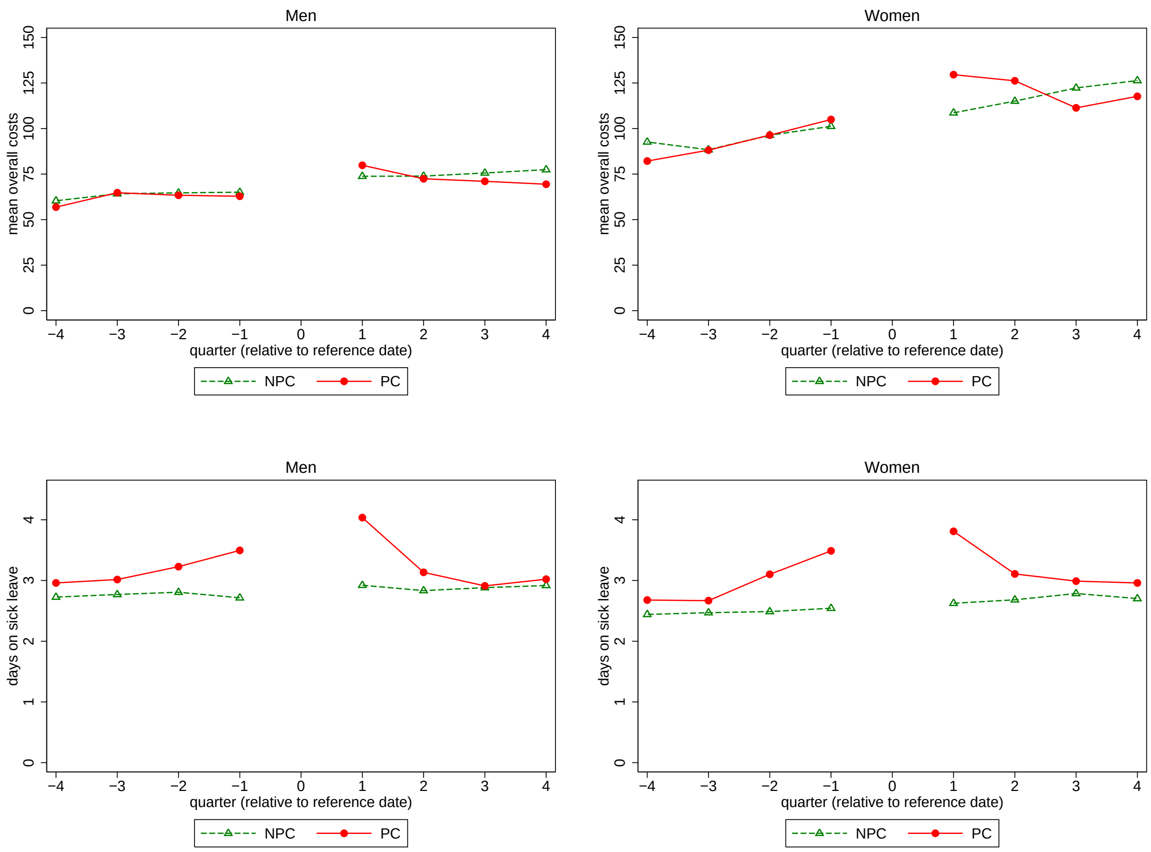
Notes: The vertical axis measures, for any given quarter since the date of (to) plant closure (reference date), the health insurance costs per worker among plant closure (PC) and non plant closure (NPC) workers. For definition of plant closure data (for PC workers) and reference date (for NPC workers) see figure 2.1. Also see table A.1 in the chapter appendix for a description of the health care services covered by the overall cost measure.

costs. We see that plant closure workers cause slightly higher health costs before the plant closure date than non–plant closure workers. After the plant closure date, health costs more than double during the first months after the plant closure and then fall again. Interestingly, we also find an increase in health costs for non-plant closure workers after the reference date. One reason for this increase is selection on workers who are employed (and hence in good health) at the date of plant closure (and most likely during the months before that date) but may become sick after that date. Therefore it is not surprising that we see an asymmetric evolution of the health care costs before and after the reference date. Moreover, the periods under consideration health costs were strongly increasing in general (in particular, in the years 2000 – 2002) which is reflected in the upward trend of health costs of non-plant closure workers after the reference (plant–closure) dates.

Overall health costs appear to be strongly affected by nonemployment resulting from plant closure. However, overall health costs can be misleading because sickness benefits are recorded differently between PC– and NPC–workers (see section 2.3). Figure 2.4

therefore reports the evolution of overall health costs excluding sickness benefits and sick leave days (which are not recorded in different ways between PC– and NPC–workers). The results regarding overall costs (excluding sick pay) indicate that for both men and women PC– and NPC–individuals are very similar before the plant closure date. Plant closure increases health care costs only slightly among men in the first quarter after the plant closure date. In the second to fourth quarter after the plant closure date, health care costs are even slightly lower for PC–men compared to NPC–men. In contrast, PC–women are incurring much higher health care costs than NPC–women in the first and second quarter after the plant closure date. In the third and fourth quarter, PC–women use slightly less health care than NPC women. Thus, results for health costs excluding sick leave suggest that the effects of unemployment on health are modest.

Figure 2.4: Overall health costs (excluding sick pay) & days on sick leave, by sex



Notes: The vertical axis measures, for any given quarter since the date of (to) plant closure (reference date), overall health costs excluding costs arising from days on sick leave (upper panel) and the average days on sick leave (lower panel) among plant closure (PC) and non plant closure (NPC) workers. For the definition of plant closure date (for PC workers) and reference date (for NPC workers) see figure 2.1. Also see table A.1 in the chapter appendix for a description of the health care services covered by the cost measure.

The bottom panel of figure 2.4 shows results for days on sick leave. For men there is only a small difference in days on sick leave the fourth and third quarter before the plant closure date. However, in the two quarters immediately before plant closure, days on sick leave tend to increase for PC–workers whereas they tend to decrease (slightly) for NPC–workers. The fact that days on sick leave increase already before plant closure might be interpreted as a sign that the identifying assumption – health status of workers does not cause plant closure – is not valid. This interpretation is unlikely to hold because workers spend on average at most 3.5 out of 91 days per quarter on sick leave. This means that it is unlikely that the deteriorating health of a small group of workers accounts for the failure of the entire firm. A second interpretation of this fact is that anticipation of plant closure is already deteriorating health among some PC–workers. Thus, an analysis that is only looking at the effects of plant closure on health status after plant closure may provide a misleading picture of the overall health effects of non–employment.

The bottom panel of figure 2.4 also shows that in the first quarter after the plant closure days on sick leave are considerably higher for individuals formerly employed in PC–firms than for NPC–individuals. But the difference in sick leave days quickly diminishes in the second quarter and vanishes completely in the third and fourth quarter after the plant closure date. The findings for women are very much in line with the findings for men. Sick leave days increase already two quarters before the plant is actually going bankrupt. The difference in sick leave days is not persistent, sick leave days among PC–women reaching approximately the same level in the third and fourth quarter after plant closure as in the third and fourth quarter before plant closure. Thus, the evidence suggests that there is a temporary and relatively weak effect of plant closure on days on sick leave.

2.5 Identification and Estimation

This section discusses our strategy to identifying the causal effect of unemployment on health (costs) that will primarily capitalize on the exogenous variation in employment status generated by the shut–down of the employer. We though start discussing identification in a simple linear regression framework.

Define y_{it} as the payments incurred by the health insurance fund that are associated with take–up of health insurance provisions of a particular individual i in the year t where $t = 0$ is the year before plant closure for workers in firms that close and the year before the plant closure date for workers in continuing establishments and $t = 1$ is the year after plant closure for the treated and the year after the plant closure date for NPCs. Let d_{it} the number of days that the individual i spent in unemployment in period t. Suppose d_{it} and y_{it} are related by the following linear relationship:

$$y_{it} = \beta_0 + \beta_1 d_{it} + x_i' \gamma + u_{it} \tag{2.1}$$

where x_i is a (column) vector of control variables measured on the plant closure date and u_{it} is an error term capturing the effect of omitted variables affecting health costs, and β_0, β_1 and γ are parameters to be estimated, the parameter of main interest being β_1. The control variables contained in x_i are: age, blue collar status, tenure in the current firm, firm size, and earnings per day worked (wage), a vector of industry dummies, region dummies, and time dummies (a set of plant closure month and plant closure year controls). Moreover, estimates are performed separately by gender because labor market attachment differs strongly between women and men. Industry and time controls are important because plant closure risk is strongly seasonal, and it differs by industry. Time controls include a dummy for the year and the month of the plant closure date to account for the fact that plant closures are more likely in December than in other months of the year. Note that for workers leaving plant closure firms early – "early leavers" – the plant closure / reference date is the date at which the worker is leaving the firm rather than the date at which the plant closes. Region controls capture the location of the employer (inside upper Austria, outside upper Austria, or unknown).

Note that the OLS estimate of β_1 in equation (2.1) is potentially biased because of endogeneity of the unemployment variable d_{it}. Days spent in unemployment d_{it} are likely correlated with u_{it} because a bad health status (that leads to high health costs) may also affect the duration that an individual spends in unemployment.[20]

2.5.1 Plant Closure as an Instrument

To tackle the problem of endogeneity we use an instrumental variable approach. This approach utilizes variation in the treatment variable d_{it} which is generated by some exogenous (with respect to health) factor. Hence we need to think of a situation where variation in unemployment is not driven by individuals' health status. This chapter uses employment in a plant closure firm as the instrument for the number of days in unemployment during the year that follows a job loss. The idea is that employment in a plant closure firm is closely correlated with unemployment in the subsequent period d_{i1} but unrelated to a worker's (ex–ante) health status y_{i0}.

We define z_i as a binary variable that equals 1 if a person is employed in a plant closure firm at the plant closure date and equals 0 if a person is employed in a firm that

[20] A similar endogeneity problem has been discussed in studies estimating the causal effect of education on health (Chevalier and Feinstein, 2006).

continues to exist after the plant closure date. In order to assess if employment in a plant closure firm is a valid instrument for the causal effect of nonemployment on the subsequent health status, several assumptions have to be satisfied (Angrist and Imbens, 1995, Angrist *et al.*, 1996). The first assumption concerns the ignorability (i.e. the randomness) of the instrument. This first assumption essentially states that the instrument can be viewed as randomly assigned, so that both potential treatments and potential outcomes do not differ between the two sub-samples. This is a strong assumption in our case, as we cannot plausibly assume a–priori randomness of the instrument (see section 2.4 above) as there are both differences in the probability of being struck by a plant closure between different firms and also between different individuals (because firms with different probabilities of shut–down have not necessarily the same composition of their work force).

The central identifying assumption of our approach is that plant closure is ignorable conditional on observed (individual and firm) characteristics x_i. The idea is that the health status of a worker does not lead to the firm going bankrupt once firm size, firm industry, and worker age, tenure, and gender have been taken into account. This assumption is essentially an exclusion restriction, that is, the instrument must not have any direct effect on health costs. The assumption is that job–loss due to plant closure does not directly affect the health status of a worker. Essentially, we assume that a worker who finds a new job immediately does not suffer from health problems and hence does not take up additional health insurance provisions. Any effect on health costs comes via days spent in unemployment. In principle, plant closure might affect health in other ways than via unemployment. For instance, areas with many plant closures might experience reduction in pollution levels thus benefitting health in these areas. Alternatively, tighter local budgets might imply deteriorating quality of health care negatively affecting health. We believe that it is unlikely that such spillovers give rise to direct effects of plant closure on health for two reasons. First, our descriptive analysis indicate that plant closures are small compared to the average employer within a region. Thus, plant closures are unlikely to generate regional spillover effects. Second, regional spillover effects would also be affecting the control group. This suggests that regional spillovers do not bias our estimates of the effects of unemployment on health.

We parametrically control for x_i using two stage least squares. Clearly, the assumption that z_i is ignorable conditional on x_i can not be tested directly. However, it turns out that we can assess the plausibility of this assumption by exploiting the panel nature of our dataset.[21] Specifically, we estimate:

$$y_{i0} = \pi_0 + \pi_1 z_i + x_i'\theta + \epsilon_{i0} \tag{2.2}$$

This regression tests whether plant closure workers are similar with respect to health care costs in the year before the plant closure date.[22] Our results support the claim that ignorability is a plausible assumption for men but less so for women. We address the problem with non-balanced health costs in the period before plant closure by introducing pre-plant closure health status as an additional control variable.

The second assumption is that the instrument must affect the treatment intensity. Working in a plant closure firm at the plant closure date must increase unemployment duration during the year following the shut-down of the plant for at least some workers. As we show in section 2.4, working in a plant closure firm indeed has a huge impact on unemployment. Hence the empirical evidence shows that this assumption is satisfied.

The third assumption postulates that the instrument has to affect all individuals in the same way (monotonicity). This assumption states that, for each individual, the (potential duration of unemployment during the year after the plant closure date is longer when the individual works in a firm that closes due to bankruptcy on the plant closure day than when the individual works in a regular firm on the plant closure day. Although this assumption is not verifiable (because it involves potential and thus fundamentally unobserved outcomes), it has a testable implication in the case of a non–binary treatment, in that the empirical cumulative distribution functions of the treatment variable does not cross for the two sub-samples (see Angrist and Imbens, 1995).

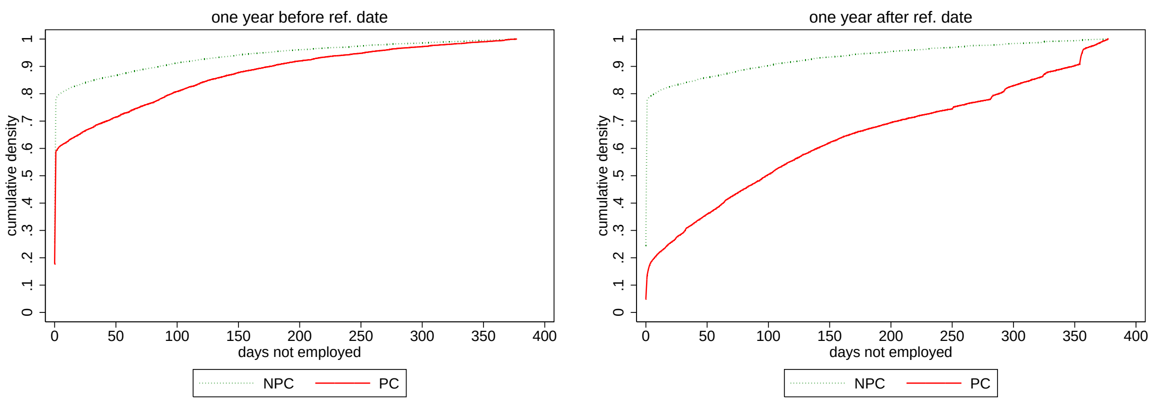
Figure 2.5 shows the empirical cumulative density of days not employed within the year preceding or following the plant closure date, respectively. As the upper figure shows, there is already a difference in the number of days not employed between the two groups within the year preceding the plant closure date (which is already evident in figure 2.2 above). But also note that this difference mainly manifests itself in the lower domain of the variable and that the difference gets smaller for longer durations of nonemployment. The right panel of figure 2.5 shows the same graph for the year just following the plant closure / reference date. First note that the graph is essentially the same for the NPC-group (compared to the year before the plant closure / reference date),

[21] An alternative approach to exploiting the panel nature of our dataset is to estimate a worker fixed effects specification or a difference–in–difference specification. Results based on these approaches are qualitatively similar to those reported in this chapter. The advantage of our approach is that it allows us to investigate the plausibility of the conditional ignorability assumption using data on the year before the plant closure date.

[22] Note that estimating model (2.2) in period $t = 1$ yields the reduced form – or "intention to treat" – effect of the instrument on health care costs.

and thus there is practically no change in the distribution of the endogenous variable for these individuals. Compare this to the large shift in the empirical distribution for the treatment group. Although we see that our instrument has the largest impact on the probability for somewhat shorter durations of nonemployment, there is also a large increase in the probability of longer durations of nonemployment. This evidence does not contradict the assumption that the plant closure event affects the days in non-employment monotonically.

Figure 2.5: Days not employed (cdf), one year before (after) the PC date



Notes: The vertical axis measures the fraction of workers non–employed (unemployed or out of labor force) for x days or less during the year prior to (upper panel) and after (lower panel) the plant closure (reference date), separately for PC and NPC workers.

Angrist and Imbens (1995) show that, if all three of the above mentioned assumptions hold, the 2SLS estimator measures the average causal effect of non–employment on health care costs for individuals which are induced to remain non–employed longer because they have been employed in a plant that closed. The average causal effect depends on both the instrument used and on the distribution of the treatment variable. This means that any estimated causal effect does not necessarily coincide with health effects of nonemployment resulting from other sources of job–loss.

2.6 Econometric Results

2.6.1 Ex–ante Differences in Health

Our aim is to study the causal effect of joblessness on public health costs that is based on a comparison of non–employment experiences of plant-closure and non-plant closure workers. This is a reliable identification strategy if the two groups do not differ with respect to their health status ex ante, that is if their health status before the shut–down of the firm is identical. To get a first hint whether this is indeed the case, we run a simple OLS regression on health costs in the year *before* the plant–closure date on the set of control variables and include the plant–closure status as an additional control variable.

Table 2.3 includes the treatment dummy (taking on the value 1 for plant closure workers, and 0 for non–plant closure workers) into this regression for males. We find a positive point estimate for overall health costs (column 1) suggesting that plant-closure workers incur higher health care costs than non–plant closure workers. However, the coefficient is not statistically different from zero. This suggests that our assumption of no differences in average health status between the two groups of workers cannot be rejected by the data. This conclusion remains unchanged if we look at different subcategories of health costs. In all cases the standard error is much larger than the point estimate indicating no significant differences between the two groups for health costs due to sickness benefits, doctor consultations, hospitalizations and drug prescriptions. The only exception is when we look at the number of days on sickness benefits (last column). Here we see that there is a significant and quantitatively important differences between PC– and NPC–workers. This finding is in line with results in figure 2.4.

The situation is similar for women as far as overall health costs are concerned (see table 2.4). We find a positive point estimate which larger than that of males but which is also not statistically different from zero. When we split up costs in subcategories for different types of health insurance provisions, we find that female PC–workers cause more costs due to doctor consultations (although quantitatively this effect is not very large – comparing the point estimate with the mean of the dependent variable at the bottom of table 2.4 shows that female PC-workers accumulate roughly 5 percent higher costs that their NPC–counterparts). Just like for males, we see that also for females the number of days on sickness benefits are higher for PC–workers than for NPC-workers. Quantitatively, the difference is quite high, amounting to more than 20 percent higher sickness days than the average worker. There is no significant difference between PC- and NPC-workers in public health costs associated with the consumption of medical drugs. Moreover, we see that the PC–coefficient for hospitalization costs is close to significant and quantitatively important. In contrast, drugs and overall costs excluding sickness benefits do no show any significant impact on overall health costs.

Tables 2.3 (for men) and 2.4 (for women) also display the partial correlations between health care costs with the control variables. Column 1 shows that there is an inverse u–shaped relationship in health care costs with respect to age, tenure and firm size but a u–shaped relationship between health care costs and income per day. Columns 2 reports results for the costs of medical treatments. The analysis reveals that there is a u-shaped relationship between costs due to medical treatments and age, no relationship between

Table 2.3: OLS–estimates, one year *before* plant closure (men)

	Overall costs (incl. sick pay)	Overall costs (excl. sick pay)	Sickness leave	Consultations	Hospitalisations	Drugs	Days on sick leave
PC	22.822	12.433	10.388	−1.134	11.844	1.723	1.320**
	(50.381)	(11.565)	(46.976)	(2.051)	(8.957)	(3.738)	(0.442)
Age (in years)	57.304***	−7.656*	64.960***	−4.946***	1.365	−4.074**	−0.366***
	(15.750)	(3.358)	(14.709)	(0.538)	(2.613)	(1.497)	(0.099)
Age/10 squared	−39.323*	22.852***	−62.175***	9.314***	4.482	9.055***	0.739***
	(19.547)	(4.450)	(18.237)	(0.731)	(3.380)	(2.114)	(0.128)
Tenure (in years)	216.004***	13.066	202.938***	−1.228	9.897	4.397	1.278***
	(59.619)	(11.073)	(55.280)	(1.834)	(9.103)	(3.972)	(0.373)
Tenure squared	−29.740**	−0.731	−29.009***	0.695*	−1.388	−0.037	−0.176**
	(9.430)	(1.920)	(8.669)	(0.335)	(1.506)	(0.788)	(0.063)
Blue collar worker	−58.225	−24.257*	−33.968	−2.543	−9.819	−11.896*	4.504***
	(48.234)	(11.381)	(44.667)	(1.972)	(8.276)	(5.636)	(0.351)
Size of firm	0.090***	0.022*	0.068*	0.001	0.021**	−0.000	0.002***
	(0.025)	(0.010)	(0.027)	(0.002)	(0.006)	(0.003)	(0.000)
Size of firm/10 squared	−0.001**	−0.000	−0.000*	−0.000	−0.000**	0.000	−0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Wage (in 100 €)	−10.447***	−1.091***	−9.356***	0.126***	−1.069***	−0.147	−0.026**
	(1.799)	(0.256)	(1.701)	(0.033)	(0.196)	(0.123)	(0.008)
Wage/10 squared	1.305***	0.104*	1.201***	−0.024***	0.112**	0.017	−0.001
	(0.294)	(0.050)	(0.277)	(0.007)	(0.035)	(0.029)	(0.001)
Mean (dep. var.)	455.497	253.028	202.469	81.270	115.658	56.100	11.464
St. dev. (dep. var.)	2918.971	723.886	2662.438	124.490	574.983	305.577	20.243
n	33,352	33,352	33,352	33,352	33,352	33,352	33,352
R^2	0.023	0.040	0.017	0.125	0.017	0.019	0.092
Industry dummies	✓	✓	✓	✓	✓	✓	✓
Regional dummies	✓	✓	✓	✓	✓	✓	✓
Time dummies	✓	✓	✓	✓	✓	✓	✓

Notes: *, **, *** denotes statistical significance on the 5%, 1% and 0.1% level, respectively. Robust standard errors (clustered by firm) in parentheses. The dependent variable in column 1 is overall health costs of a worker during the year before the plant closure (reference) date, overall costs excluding sick pay in column 2, subcategories of health costs in columns 3 to 6, and days on sick leave in column 7 (see table A.1 in the chapter appendix). PC is a dummy variable which equals 1 for PC workers and 0 otherwise. All control variables are measured at the last date before plant closure (before the reference date). There are 9 industry dummies, 28 regional dummies, and 15 time dummies (year and month).

Table 2.4: OLS–estimates, one year *before* plant closure date (women)

	Overall costs (incl. sick pay)	Overall costs (excl. sick pay)	Sickness leave	Consultations	Hospitalisations	Drugs	Days on sick leave
PC	50.278	30.707	19.571	8.704**	24.490	−2.486	2.236***
	(39.889)	(17.209)	(29.195)	(2.974)	(13.603)	(7.269)	(0.444)
Age (in years)	0.450	−9.591*	10.041	−0.287	−7.033*	−2.271	−0.800***
	(8.529)	(3.942)	(6.514)	(0.792)	(2.896)	(1.628)	(0.109)
Age/10 squared	18.744	25.379***	−6.635	4.327***	13.467***	7.585***	1.292***
	(11.829)	(5.457)	(9.105)	(1.101)	(3.971)	(2.253)	(0.156)
Tenure (in years)	32.197	−6.213	38.409	−2.722	1.012	−4.503	−0.173
	(40.776)	(15.459)	(34.631)	(3.032)	(12.305)	(5.996)	(0.366)
Tenure squared	−0.311	4.003	−4.314	1.080*	1.093	1.830	0.071
	(6.682)	(2.675)	(5.590)	(0.532)	(2.162)	(1.052)	(0.064)
Blue collar worker	32.483	8.922	23.561	−0.875	21.426*	−11.628*	5.276***
	(32.395)	(14.382)	(25.861)	(2.989)	(10.810)	(5.511)	(0.470)
Size of firm	0.037	0.023	0.014	0.003	0.017*	0.003	0.001*
	(0.024)	(0.012)	(0.016)	(0.003)	(0.007)	(0.004)	(0.001)
Size of firm/10 squared	−0.000	−0.000*	−0.000	−0.000	−0.000*	−0.000	−0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Wage (in 100 €)	−3.597***	−0.197	−3.400***	0.176***	−0.332	−0.041	0.016*
	(1.001)	(0.287)	(0.838)	(0.049)	(0.193)	(0.153)	(0.007)
Wage/10 squared	0.539**	−0.041	0.580***	−0.058***	0.024	−0.007	−0.007***
	(0.192)	(0.063)	(0.158)	(0.012)	(0.044)	(0.031)	(0.002)
mean dep. var.	469.108	377.100	92.008	155.789	136.051	85.260	10.373
s.d. dep. var.	1725.697	818.573	1314.157	172.825	574.091	411.819	18.947
n	19,243	19,243	19,243	19,243	19,243	19,243	19,243
R^2	0.022	0.041	0.010	0.132	0.014	0.015	0.071
Industry dummies	✓	✓	✓	✓	✓	✓	✓
Regional dummies	✓	✓	✓	✓	✓	✓	✓
Time dummies	✓	✓	✓	✓	✓	✓	✓

Notes: *, **, *** denotes statistical significance on the 5%, 1% and 0.1% level, respectively. Robust standard errors (clustered by firm) in parentheses. The dependent variable in column 1 is overall health costs of a worker during the year before the plant closure (reference) date, overall costs excluding sick pay in column 2, subcategories of health costs in columns 3 to 6, and days on sick leave in column 7 (see table A.1 in the chapter appendix). PC is a dummy variable which equals 1 for PC workers and 0 otherwise. All control variables are measured at the last date before plant closure (before the reference date). There are 9 industry dummies, 28 regional dummies, and 15 time dummies (year and month).

medical costs and tenure, and a positive and linear relationship between medical costs and firm size. The result for age is very much in line with the fact that older individuals tend to incur higher costs due to medical treatments. Column 3 reports the results for sickness benefits. Interestingly, these results are in line with a standard income equation. This is not surprising since sickness benefits act as income replacement.

The results in table 2.3 and table 2.4 indicate that the plant closure is not directly related to health care costs before plant closure for men. In contrast, the picture is somewhat less favorable for women. While only doctor visits and days on sick leave are significantly higher for PC–women at the 5 percent level, hospitalizations are also significantly higher at the 10 percent level. This means that it will be important to assess the sensitivity of our results to additionally controlling for health care costs or to assessing the changes in health care costs.

2.6.2 The Causal Effect of Unemployment on Health Costs

The Effect of Plant Closure on Nonemployment

Table 2.5 shows the first–stage effect of plant closure on the number of days spent in non–employment. That is, we run the the following regression, separately for men and women:

$$d_i = z_i\gamma + x_i'\delta + \epsilon_i \tag{2.3}$$

We see that the instrument is very strong, i.e. the first stage F–statistic is 261.65. The PC–coefficient is highly significant (the coefficient being more than 75 times larger than the standard error) and the impact is quantitatively important. The average male plant closure worker spends 84 days more out of a job than the average non–plant closure worker. PC–women are even more strongly affected by plant closure remaining 123 days longer without work than corresponding NPC–women.

IV–Estimates

In what follows we present IV-estimates on the causal effects of non-employment after a job loss on health costs. The dependent variable (i.e. the various health indicators) now refers to the years *after* the plant closure date. To show that it is important to adopt an IV-strategy to assess a causal impact of non-employment on health indicators, the following tables list both the IV-estimator of number of days in non-employment, and the corresponding OLS-estimator from a regression of health indicators after the plant closure date on the number of days in non-employment during the year after the plant

Table 2.5: First–stage regressions

	Men	Women
PC	83.755***	122.582***
	(1.068)	(1.685)
Age (in years)	−3.193***	−6.565***
	(0.291)	(0.417)
Age/10 squared	5.176***	8.845***
	(0.372)	(0.558)
Tenure (in years)	−6.463***	−4.530*
	(1.189)	(1.776)
Tenure/10 squared	67.041**	35.852
	(20.836)	(30.617)
Blue collar worker	2.549*	−5.322***
	(1.053)	(1.550)
Size of firm	0.001	0.002
	(0.001)	(0.001)
Size of firm/10 squared	−0.000	−0.000**
	(0.000)	(0.000)
Agriculture	11.756*	11.610
	(5.851)	(9.031)
Mining	−5.957	−5.300
	(5.790)	(17.975)
Construction	−6.205***	−8.652*
	(1.625)	(3.623)
Manufacturing	−12.859***	−5.190**
	(1.472)	(1.876)
Transportation, utilities	−15.344***	−4.513
	(2.147)	(4.448)
Wholesale treade	−7.880***	−7.286**
	(2.113)	(2.775)
Retail trade	−8.926***	−15.489***
	(2.193)	(2.169)
Information, finance	−4.342*	−8.842***
	(1.856)	(2.623)
Wage (in 100) €	−0.678***	−0.495***
	(0.019)	(0.027)
Wage/10 squared	0.104***	0.103***
	(0.004)	(0.007)
Constant	187.892***	256.505***
	(10.928)	(20.758)
Regional dummies included?	✓	✓
Time dummies included?	✓	✓
n	33,352	19,243
R^2	0.317	0.339
F–statistic	261.652	169.575
p–value (F–statistic)	0.000	0.000

Notes: *, **, *** denotes statistical significance on the 5%, 1% and 0.1% level, respectively. Dependent variable is days not employed one year after the plant closure date. There are 28 regional dummies and 15 time dummies (year and month).

closure date.[23]

Table 2.6 presents the IV-estimator for overall health costs for men. We see that an additional day in non-employment causes an increase in overall public health costs by 4,85 Euros. This is a large number. To see this recall that, before the plant closure date, the average health costs of PC– and NPC–worker amount to about 500 Euros per year and worker (see Table 2.1). The first-stage regression reveals that a plant-closure event raises days in non-employment by more than 83 days for males (and by more than 120 days of females). Taken together, our estimate suggests that public health costs of the typical male PC-worker increase by more than 400 Euros (or by 80 percent!).

However, columns 2 and 3 of table 2.6 show that the increase is almost entirely due to higher sickness benefits payments and not due to costs associated with increased take–up of health care provisions. The IV–coefficient in column 2 shows that the causal effect of non–employment on health–related take–up of insurance provisions (consultations, hospitalizations, drugs) is not only quantitatively very small but also statistically insignificant. In contrast, column 3 reveals that the increase in public health costs is entirely due to an increase in sickness payments which, for jobless workers, have now to be borne by the public health insurance system rather than by the employer. Columns 4 to 7 decompose this impact into various health provision subcategories. It turns out that that non-employment has no significant impact on hospitalization costs and on costs associated with the consumption of medical drugs. Interestingly, we find a significant (though quantitatively small) reduction in health costs due to doctor visits. This suggests that doctor consultations cannot be strongly driven by a lower opportunity costs of time for the non–employed.

In panel B of Table 2.6, we show the corresponding estimates for females. The picture is very similar. Overall health costs increase, but the bulk of this increase is due to increased sickness benefit payments. When we exclude these payment from the overall health costs, the coefficient becomes small and statistically insignificant. However, in contrast to males, non-employment causes additional health costs for females in various subcategories, in particular due to increased hospitalization costs and due to an increased number of sickness days. Similarly to males, however, doctor consultation decreases with more days in non–employment.

[23]Regressing health indicators on days not employed before the plant closure date yields qualitatively similar results with two exceptions (see table A.3 in the appendix). Drug prescriptions for men are not significantly positively related to days not employed in the year before plant closure. Also, hospitalization costs for women are much less strongly positively related with days not employed. This is due to pregnancy costs which are much more important after plant closure than before plant closure.

Table 2.6: Comparison between IV and OLS, one year *after* plant closure date

	Overall costs	Overall costs	Sickness leave	Consultations	Hospitalizations	Drugs	Days on sick leave
A. Men							
IV	4.850***	0.151	4.698***	−0.073*	0.146	0.078	0.007
	(1.065)	(0.155)	(1.010)	(0.032)	(0.119)	(0.069)	(0.006)
OLS	9.035***	0.980***	8.055***	0.004	0.826***	0.150**	0.035***
	(0.872)	(0.107)	(0.830)	(0.012)	(0.087)	(0.047)	(0.003)
n	33,352	33,352	33,352	33,352	33,352	33,352	33,352
B. Women							
IV	2.866***	0.346	2.521***	−0.079*	0.479**	−0.054	0.019***
	(0.812)	(0.190)	(0.713)	(0.032)	(0.160)	(0.058)	(0.005)
OLS	5.737***	1.660***	4.077***	0.142***	1.420***	0.099**	0.030***
	(0.718)	(0.129)	(0.663)	(0.021)	(0.105)	(0.030)	(0.003)
n	19,243	19,243	19,243	19,243	19,243	19,243	19,243
Control variables	✓	✓	✓	✓	✓	✓	✓
Industry dummies	✓	✓	✓	✓	✓	✓	✓
Regional dummies	✓	✓	✓	✓	✓	✓	✓
Time dummies	✓	✓	✓	✓	✓	✓	✓

Notes: *, **, *** denotes statistical significance on the 5%, 1% and 0.1% level, respectively. Robust standard errors (clustered by firm) in parentheses. The table reports the coefficient of the (instrumented) variable "days not employed", which measures the number of days not in employment during the year after plant closure (after the reference date). The F–value of the first–stage regression is 261.65 (p–value = 0.000) for men and 169.57 (p–value = 0.000) for women. See also the notes of table 2.3.

Comparison Between IV and OLS

For means of comparison, table 2.6 also shows the OLS–estimates. Both for males and for females we see a consistent picture. OLS–estimates are much higher than the IV–estimates. All coefficients shown in table 2.6 indicate that more days in non–employment are associated with higher health costs, the only exception being doctor consultations for males. While the IV–coefficient rules out reverse causality by the assumption that plant–closure is unrelated to worker health, the OLS–coefficients may in addition be driven by reverse causality, as less workers are more likely to become unemployed. We see that not appropriately accounting for this reverse causality may have a substantial impact on the estimated coefficient. This underlines the importance of our instrumental variable approach for identification.

2.6.3 Robustness Checks

Additional Control Variables

One could argue that our IV–coefficients are biased as the health indicators of PC– and NPC–workers are not completely identical before the plant closure event. To shed light on this issue and to check the robustness of our previous estimates, table 2.7 reports the IV–coefficients from regressions that enrich the set of control variables. In particular, to account for differences in employment performance and health indicators between PC– and NPC–workers *before* the date of plant closure we include as additional regressors the number of days in non–employment and the total health costs during the year prior to that date. It turns out that our main results presented in table 2.6 remain unchanged when we perform this robustness test. In particular, both for males and for females the increase in public health costs is driven by sickness benefits payments rather than an increase in the health status of non–employed workers as measured by the health costs for hospitalizations, doctor consultations, and medical drugs. For males and for females we find that doctor consultation decrease rather than increase when the individual experiences more days in non–employment. For females but not for males, we see an increase in the number of sickness days. In sum, table 2.7 reproduces almost exactly our main results presented in table 2.6.

Changes in Health Costs

Table 2.8 shows the main results using the change in health outcomes as the dependent variable. This specification allows for differences in health outcomes that are not captured with our control variables. Results for men indicate that the strong effects on overall health costs are primarily due to the effects of non–employment on sick leave

Table 2.7: IV–estimates with additional control variables, one year *after* plant closure date

	Overall costs (incl. sick pay)	Overall costs (excl. sick pay)	Sickness leave	Consultations	Hospitalizations	Drugs	Days on sick leave
A. Men							
Days not employed	4.630***	0.098	4.531***	−0.089**	0.100	0.087	0.002
	(1.051)	(0.142)	(1.003)	(0.029)	(0.117)	(0.063)	(0.005)
n	33,352	33,352	33,352	33,352	33,352	33,352	33,352
R^2	0.063	0.169	0.043	0.154	0.050	0.224	0.166
B. Women							
Days not employed	2.653***	0.219	2.434***	−0.113***	0.402**	−0.070	0.012**
	(0.781)	(0.163)	(0.709)	(0.032)	(0.151)	(0.052)	(0.005)
n	19,243	19,243	19,243	19,243	19,243	19,243	19,243
R^2	0.098	0.212	0.049	0.173	0.088	0.230	0.171
Control variables	✓	✓	✓	✓	✓	✓	✓
Nonemployment before	✓	✓	✓	✓	✓	✓	✓
Health costs before	✓	✓	✓	✓	✓	✓	✓
Industry dummies	✓	✓	✓	✓	✓	✓	✓
Regional dummies	✓	✓	✓	✓	✓	✓	✓
Time dummies	✓	✓	✓	✓	✓	✓	✓

Notes: *, **, *** denotes statistical significance on the 5%, 1% and 0.1% level, respectively. Robust standard errors (clustered by firm) in parentheses. The F–value of the first–stage regression is 261.65 (p–value = 0.000) for men and 169.57 (p–value = 0.000) for women. The table corresponds to table 2.3, except that the following variables are additionally controlled for: days not employed one year before the plant closure (reference date), overall health costs in the year before the plant closure (reference date), number of days on sick leave and sickness benefits in the year before the plant closure (reference) date.

pay. In contrast to the main results, estimates that are explaining the change in health outcomes suggest that doctor consultations are positively affected by non–employment. This change in results can probably be explained by the fact that PC–men are consulting doctors slightly less frequently than NPC–men before plant closure (see table 2.3). Panel B reports results for women. There are two important differences between the results that account for pre existing differences in health outcomes between PC– and NPC–women and the main results that only focus on the time period after plant closure. The new results suggest that hospitalizations are only marginally significantly positively affected (z–value of about 1.95) and that there are no effects on days on sick leave. The other results are qualitatively very similar to our main results.

The robustness checks suggest that the main results are not sensitive to controlling for differences in health costs before plant closure. However, the main results are sensitive to measuring effects on changes in health costs rather than on levels of heath care costs. The key question is whether this sensitivity in results is due to permanent or temporary differences in health outcomes before plant closure. Permanent differences suggest that the main identification strategy fails because these permanent differences can not be caused by non–employment. In contrast, temporary differences in health outcomes can arise if health reacts in anticipation of plant closure. We discuss possible anticipation effects in more detail below (see figures 2.6 and 2.7).

2.6.4 Detailed Results

Hospitalizations

Tables 2.9 and 2.10 look in more detail at hospitalization costs (table 2.9) and on public health costs associated with the consumption of medical drugs (table 2.10).

Panel A of table 2.9 shows that, for males, hospitalization costs associated with cancer, stroke, respiratory ailments and other hospitalization are not affected by days in non–employment. However, we see that hospitalizations due to mental illnesses are significantly affected by days in non-employment. This is in line with previous research that has emphasized that, in the short run, the experience of job loss and associated non-employment may be predominantly showing up in case of mental illnesses where physical illnesses manifest themselves only over a longer term. Panel B of table 2.9 shows that, for females, days in non–employment cause a significant increase in overall hospitalization costs and that this increase is mainly due to hospitalization due to pregnancy and hospitalizations for other reasons. The results that pregnancies increase following a job loss is consistent with the hypothesis that there are fertility timing considerations of women. If a mother plans to have a child, she will choose the timing of a birth when the

Table 2.8: IV–estimates, difference in health measures (after minus before plant closure)

	Overall costs (incl. sick pay)	Overall costs (excl. sick pay)	Sickness leave	Consultations	Hospitalizations	Drugs	Days on sick leave
A. Men							
Days not employed	4.577***	0.003	4.574***	0.059*	0.004	0.058	−0.008
	(1.196)	(0.160)	(1.162)	(0.024)	(0.144)	(0.055)	(0.005)
n	33,352	33,352	33,352	33,352	33,352	33,352	33,352
R^2	0.019	0.003	0.019	0.013	0.002	0.003	0.001
B. Women							
Days not employed	2.456**	0.095	2.361***	−0.150***	0.279	−0.034	0.001
	(0.778)	(0.160)	(0.704)	(0.030)	(0.143)	(0.044)	(0.005)
n	19,243	19,243	19,243	19,243	19,243	19,243	19,243
R^2	0.018	0.007	0.015	0.005	0.011	0.002	0.007
Control variables	✓	✓	✓	✓	✓	✓	✓
Industry dummies	✓	✓	✓	✓	✓	✓	✓
Regional dummies	✓	✓	✓	✓	✓	✓	✓
Time dummies	✓	✓	✓	✓	✓	✓	✓

Notes: *, **, *** denotes statistical significance on the 5%, 1% and 0.11% level, respectively. Robust standard errors (clustered by firm) in parentheses. The F–value of the first–stage regression is 261.65 (p–value = 0.000) for men and 169.57 (p–value = 0.000) for women. The table corresponds to table **??**, except that the dependent variable is the difference in the sickness measure one year after the plant closure date minus the sickness measure one year before the plant closure date.

Table 2.9: Detailed results for hospitalizations, one year *after* plant closure date

	Hospitalizations	Cancer	Heart	Mental	Other	Pregnancy	Respiratory	Stroke
A. Men								
IV	0.146	−0.018	0.015	0.134*	0.011		0.017	−0.013
	(0.119)	(0.030)	(0.019)	(0.066)	(0.081)		(0.037)	(0.015)
OLS	0.826***	0.123***	0.024*	0.162***	0.455***		0.038	0.024*
	(0.087)	(0.027)	(0.011)	(0.040)	(0.055)		(0.034)	(0.010)
n	33,352	33,352	33,352	33,352	33,352		33,352	33,352
B. Women								
IV	0.479**	0.055	0.025	0.129	0.245*	0.203***	0.029	−0.005
	(0.160)	(0.047)	(0.038)	(0.093)	(0.096)	(0.052)	(0.015)	(0.011)
OLS	1.420***	0.066**	0.020	0.235***	1.069***	0.838***	0.021**	0.010
	(0.105)	(0.023)	(0.023)	(0.062)	(0.064)	(0.048)	(0.008)	(0.007)
n	19,243	19,243	19,243	19,243	19,243	19,243	19,243	19,243
Control variables	✓	✓	✓	✓	✓	✓	✓	✓
Industry dummies	✓	✓	✓	✓	✓	✓	✓	✓
Regional dummies	✓	✓	✓	✓	✓	✓	✓	✓
Time dummies	✓	✓	✓	✓	✓	✓	✓	✓

Notes: *, **, *** denotes statistical significance on the 5%, 1% and 0.1% level, respectively. Robust standard errors (clustered by firm) in parentheses. The table reports the coefficient of the (instrumented) variable "days not employed", which measures the number of days not in employment during the year after plant closure (after the reference date). The F–value of the first–stage regression is 261.65 (p–value = 0.000) for men and 169.57 (p–value = 0.000) for women. See also the notes of table 2.3.

opportunity costs are low. Interestingly, also the catch-all category other hospitalizations increase for females which suggests that hospitalization costs are partly also associated with health problems. Table 2.9 again shows not only IV–coefficients but also OLS–coefficients. The picture established in Table 2.6 before is confirmed also in table 2.9. The majority of OLS–estimates is positive and significant, suggesting that more days in non–employment are associated with higher hospitalization costs. However, with the few exceptions mentioned above this is most likely the result of reverse causality.

Medical Drugs

Table 2.10 shows a similar analysis for the case of public health costs associated with the consumption of medical drugs. While overall drug costs are not significantly affected by days in non–employment, we find that, for males, the consumption of psychotropic drugs (antidepressants, etc.) treating depressive conditions significantly increases with more days in non–employment. This underlines the relevance of mental health problems as a possible effect of non–employment due to plant closure. However, we do not find a significant impact of the consumption of psychosomatic drugs , neither for males nor for females. Again, OLS–estimates suggest a positive relationship between days in non–employment and the consumption of drugs. These coefficients are significant in all subcategories but most likely driven by reverse causality.

One could argue that the absence of any substantial difference in public health costs after a plant closure does not mean that health costs are unaffected as workers could suffer from health shock in anticipation of job loss and the fear of extended periods of joblessness. Figure 2.6 shows the IV-estimates in each of the four quarters before and after plant closure. This analysis allows discussing whether health care costs increase already before job loss due to plant closure. The first row reports the effects of the days not employed in the year after plant closure date on total health care costs in each of the 4 quarters before plant closure and after plant closure.[24] Results indicate that there is no significant effect before the plant closure date but a strong and significant effect in each quarter after the reference date. Results in the second row report the effect of days not employed on days on sick leave. For both men and women, there is no significant effect in quarters four and three before plant closure. However, results clearly indicate that workers enter sick leave already two quarters before the plant closure date. This anticipation effect is significant at the 5 percent level. Moreover the effect of nonemployment on plant closure remains significantly positive in the first (men) and in the first and second (women) quarter after plant closure date. This is consistent with

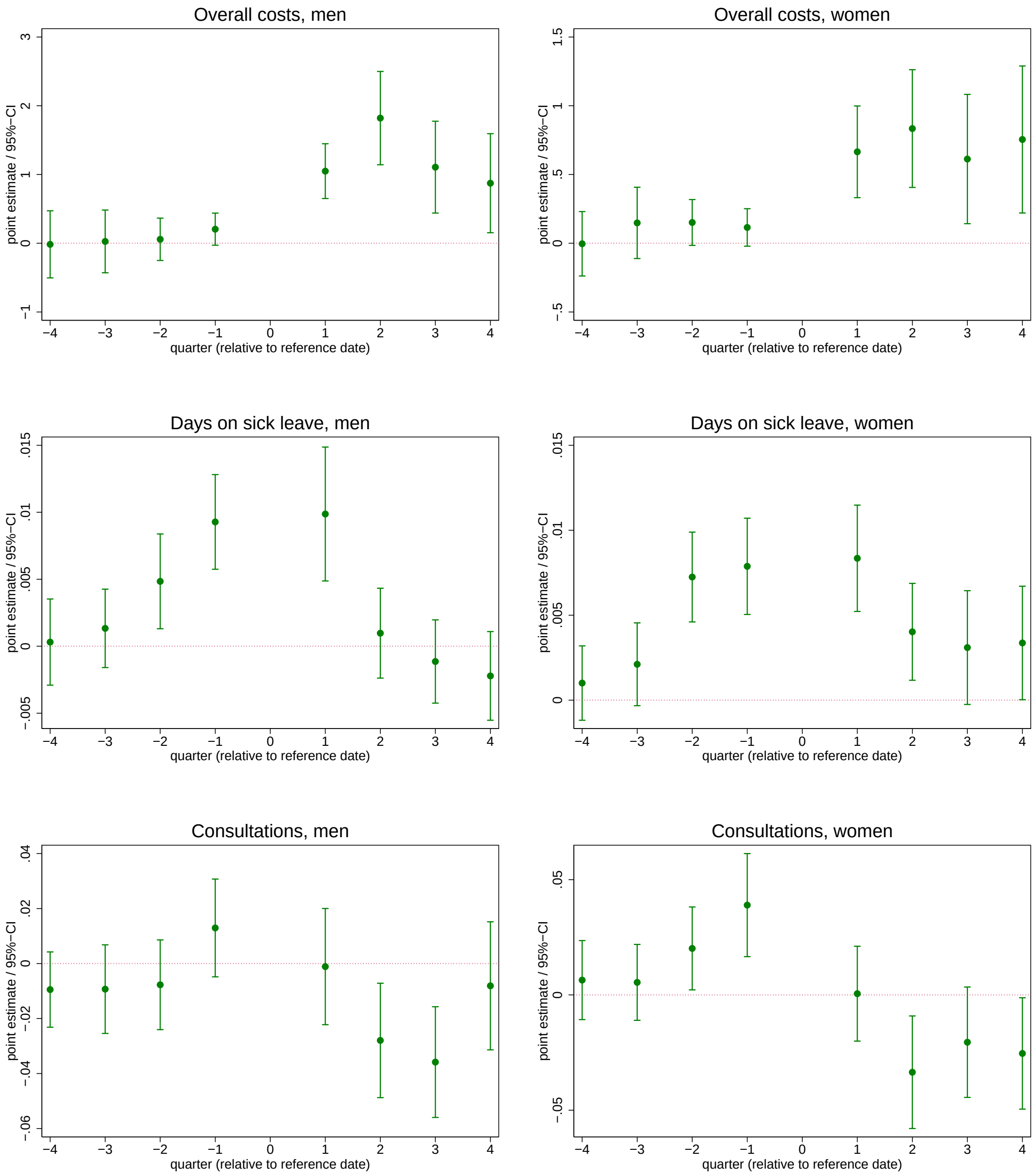
[24]Technically, this is a decomposition of the effects reported in the main analysis. Moreover, this allows assessing whether there are effects already before plant closure.

Table 2.10: Detailed results for drug prescriptions, one year *after* plant closure date

	All drugs	Specific	Psychosomatic	Psychotropic	Nonspecific
A. Men					
IV	0.078	0.017	−0.001	0.018*	0.061
	(0.069)	(0.011)	(0.006)	(0.008)	(0.067)
OLS	0.150**	0.031***	0.014***	0.017***	0.119**
	(0.047)	(0.006)	(0.003)	(0.004)	(0.046)
n	33,352	33,352	33,352	33,352	33,352
B. Women					
IV	−0.054	0.002	0.002	−0.000	−0.056
	(0.058)	(0.013)	(0.008)	(0.009)	(0.057)
OLS	0.099**	0.028***	0.010**	0.018***	0.070*
	(0.030)	(0.006)	(0.004)	(0.004)	(0.029)
n	19,243	19,243	19,243	19,243	19,243
Control variables	✓	✓	✓	✓	✓
Industry dummies	✓	✓	✓	✓	✓
Regional dummies	✓	✓	✓	✓	✓
Time dummies	✓	✓	✓	✓	✓

Notes: *, **, *** denotes statistical significance on the 5%, 1% and 0.1% level, respectively. Robust standard errors (clustered by firm) in parentheses. The table reports the coefficient of the (instrumented) variable "days not employed", which measures the number of days not in employment during the year after plant closure (after the reference date). The F–value of the first–stage regression is 261.65 (p–value = 0.000) for men and 169.57 (p–value = 0.000) for women. Specific drugs includes all psychosomatic and psychotropic drugs. See also the notes of table 2.3.

Figure 2.6: IV–estimates for selected regressors (1/2)



Notes: Each graph shows the estimated coefficient of the number days not employed one year after the plant closure (reference) date, instrumented by the dummy variable PC (equal to 1 for PC workers and 0 otherweise), and its corresponding 95% confidence interval. The dependent variable in the upper panel is overall health costs, for each of the eight quarters centered around the plant closure (reference) date. The dependent variable in the middle (lower) panel is the number of days on sick leave (health costs due to consultations). The left (right) panels show the estimated coefficients for men (women). All regressions include the full set of control variables as in Table 2.3.

an interpretation that the fear of loosing one's job can already lead to a deterioration of health.[25] The third row reports the effect of job loss due to plant closure on costs due to

[25]Note that the results for sick leave and overall costs (reflecting to a large extent sickness benefits) appear to be contradictory. Recall however, that the first up to 12 weeks of sickness benefits are paid

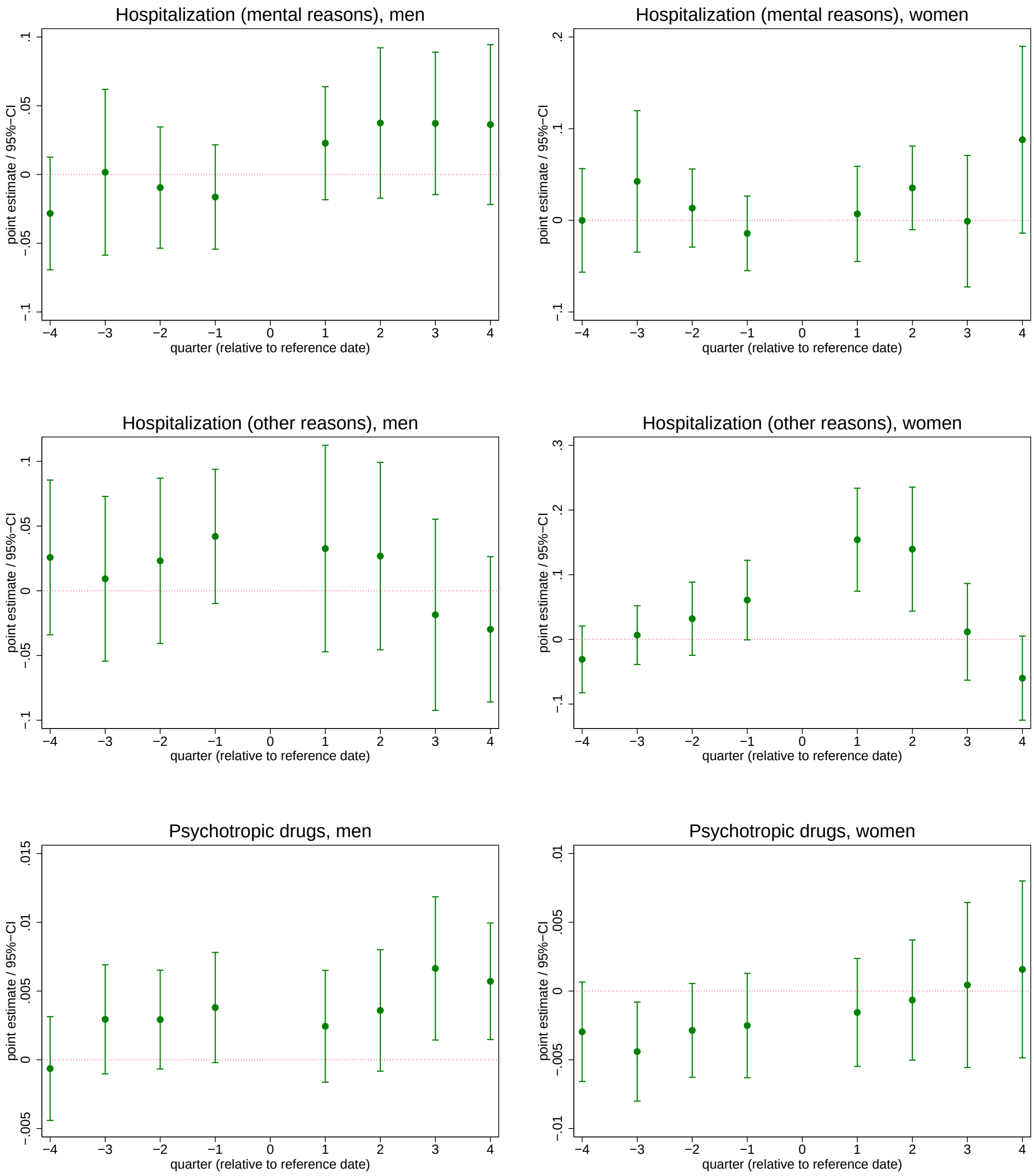
doctor visits. Results indicate that there is a significant reduction of these costs in the second and third quarter after plant closure for men. Women see doctors more frequently in the two quarters just before plant closure, but less frequently in the second quarter after plant closure. The result for women is consistent with our finding that job loss due to plant closure is associated with higher pregnancy hospitalization costs. It appears that women use periods without employment for family planning. Figure 2.7 provides detailed results for hospitalizations due to mental health or other reasons, and for psychotropic drugs. Results for mental health costs for men indicate that there is no effect of non-employment on mental health for men both before and after plant closure date. The point estimates after plant closure are, however, consistently positive explaining why the overall effect is significantly positive as we find in table 2.10. Results for women indicate that mental health is not affected by job loss due to plant closure. Recall that hospitalization due to other reasons are significantly affected by job loss due to plant closure for women. The second row in figure 2.7 indicates that there is an important upward trend in the effect of nonemployment on health care costs already before plant closure reaching marginal significance in the quarter just before plant closure. The effects of non-employment on other hospitalization costs are significantly positive in the first two quarters after the plant closure date and collapse to zero thereafter. The timing of these costs is consistent with these expenditures being indirectly related with health conditions arising due to pregnancies. The pattern of the effects on other hospitalization costs for men does not indicate any effect of nonemployment. The third row shows results for psychotropic drug prescriptions. Results for men indicate that the effects are significantly positive in the third and fourth quarter after plant closure. This suggests that mental health reacts in a relatively sluggish way to changes in the employment situation. There is no effect of nonemployment on the prescription of psychotropic drugs among women.

2.7 Conclusions

This chapter studies the causal effect of unemployment on public expenditures on health care in a typical European welfare state. Our empirical analysis focuses on the case of Austria where public health insurance is mandatory for all employees. To assess the causal relationship between individual unemployment and public health care costs we have exploited a unique data set that combines detailed information on a worker's earnings and employment history (and their firms) with detailed information on payments by the public health insurance authority associated with take-up of health care benefits

by the employer and will therefore not be recorded as sickness benefits in our data. Thus, the finding of no effect on overall costs but a strong significant effect on days on sick leave before plant closure can be explained.

Figure 2.7: IV–estimates for selected regressors (2/2)



Notes: Each graph shows the estimated coefficient of the number days not employed one year after the plant closure (reference) date, instrumented by the dummy variable PC (equal to 1 for PC workers and 0 otherweise), and its corresponding 95% confidence interval. The dependent variable in the upper panel is health costs of hospitalization due to mental reasons, for each of the eight quarters centered around the plant closure (reference) date. The dependent variable in the middle (lower) panel is health costs of hospitalization due to other reasons (health costs of psychotropic drugs). The left (right) panels show the estimated coefficients for men (women). All regressions include the full set of control variables as in Table 2.3.

(both treatment–related health care provisions such as hospitalization, doctor visits, and drug prescriptions; and sickness benefits). To tackle the problem of reverse causality – bad health may cause unemployment – we use job loss due to plant closure as an instrumental variable. Job loss due to plant closure is a meaningful instrument because such job losses are very closely associated with higher subsequent unemployment. Moreover it is very unlikely that job losses due to plant closure are caused by a worker's health.

Our empirical analysis yields several interesting results. *First*, it turns out that unemployment following a plant closure does not cause a significant increase in public health costs associated with take–up of health provisions. Public health costs due to hospitalizations and medical drugs prescription do not increase significantly, and doctor visits even fall. *Second,* while overall take-up of health provisions is not significantly affected, we find that – for males, but not for females – an increase in public health costs due to mental health problems. This result is in line with the hypothesis that, in the short run, unemployment causes mental health problems, whereas physical health is affected only in the long run. *Third,* we find that the public health costs that are associated with payments of sickness benefits strongly increase after a job loss. However, this increase in costs does not reflect a deteriorating health status of displaced workers but is mainly due to sickness benefit rules (for employed workers, employers have to bear sickness benefits, whereas for unemployed workers, the public health insurance pays these benefits). We do not find that male plant-closure workers do have more sickness *days* than non-plant closure workers. For females, however, we find a significant increase in sickness days.

In sum, our results indicate that unemployment is not associated with strong changes in health care costs arising due to treatment of medical conditions but with strong changes in sickness transfer payments. There are two lessons from this result. First, our analysis shows clearly that public health expenditures are fluctuating strongly in a country that use their health insurance to cover not only the costs of medical treatments but also to pay out sickness benefits. Second, short work career disruptions do not appear to deteriorate health in the short run. Future research should therefore focus on assessing the long-term consequences of prolonged unemployment.

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2.A Additional Tables

Table A.1: Health Indicators, Definitions

Indicator	Definition
Consultations	Includes all costs[1] arising from consultations by a physician
Drugs	Includes all costs arising from prescribed or selfmedicated drugs
Psychosomatic drugs:	Includes drugs targeted at treating psychosomatic afflictions (e.g. migraine therapeutics, antiinflammatory drugs)
Psychotropic drugs:	Includes drugs targeted at treating psychological stress (e.g. sedatives, benzodiazepins, antidepressants)
Specific drugs:	Includes psychosomatic and psychotropic drugs
Overall:	Includes all drugs
Hospitalisation	Includes costs due to hospitalisation. These costs are classified by the main diagnosis of the hospitalisation (ICD–9 codes)[2]
Cancer:	Includes ICD–9 Codes 140–239
Heart:	Includes ICD–9 Codes 391, 392.0, 393–398, 402, 404, 410–429
Mental:	Includes ICD–9 Codes 290–319, V70.1, V70.2, V71.0
Respiratory:	Incudes ICD–9 Codes 460–519
Cerebrovascular:	Includes ICD–9 Codes 430–438
Other:	Includes hospitalisation due to all other reasons
Overall:	Includes hospitalisation due to any cause
Pregnancy:	Includes ICD–9 Codes 630–676
Incapacity to Work	Includes all costs arising from being on sick leave (”Krankengeld”)
Overall costs	Includes the overall costs from consultations, drugs, hospitalisaiton, and days on sick leave

1: All variables measured in (nominal) Euros.
2: Classification largely taken from Keefe et al. (2002).

Table A.2: Descriptive statistics, background characteristics

	Men		Women	
	NPC	PC	NPC	PC
Individual characteristics				
Days not employed	22.858	55.847	24.378	34.044
	(59.882)	(85.348)	(68.013)	(79.404)
Age	36.928	35.671	36.137	33.267
	(10.374)	(10.264)	(10.096)	(11.044)
Blue collar worker	0.564	0.740	0.315	0.360
	(0.496)	(0.439)	(0.464)	(0.480)
Wage (in 100 €)	237.375	195.272	158.165	126.329
	(94.611)	(103.692)	(82.596)	(81.566)
Tenure in last five years	3.198	1.800	3.252	2.508
	(1.917)	(1.791)	(1.850)	(1.770)
Size of firm (one year before)	669.972	56.537	1075.416	74.154
	(2005.737)	(74.868)	(2954.541)	(99.240)
Industry (employer)				
Agriculture	0.005	0.006	0.005	0.006
	(0.072)	(0.076)	(0.070)	(0.075)
Mining	0.006	0.002	0.001	0.000
	(0.080)	(0.049)	(0.038)	(0.021)
Construction	0.129	0.299	0.028	0.062
	(0.336)	(0.458)	(0.166)	(0.241)
Manufacturing	0.387	0.291	0.200	0.263
	(0.487)	(0.454)	(0.400)	(0.440)
Transportation	0.059	0.058	0.025	0.013
	(0.236)	(0.233)	(0.155)	(0.112)
Wholesale trade	0.065	0.044	0.067	0.068
	(0.247)	(0.205)	(0.251)	(0.252)
Retail trade	0.057	0.047	0.120	0.150
	(0.232)	(0.212)	(0.325)	(0.357)
Information, finance	0.094	0.079	0.086	0.043
	(0.292)	(0.269)	(0.280)	(0.203)
Other services	0.107	0.073	0.338	0.339
	(0.309)	(0.259)	(0.473)	(0.473)
Industry unknown	0.090	0.102	0.130	0.056
	(0.286)	(0.303)	(0.336)	(0.230)
Region (employer)				
Outside Upper Austria	0.140	0.159	0.099	0.068
	(0.347)	(0.357)	(0.299)	(0.252)
Inside Upper Austria	0.780	0.814	0.779	0.924
	(0.414)	(0.389)	(0.415)	(0.266)
Region unknown	0.079	0.036	0.122	0.008
	(0.270)	(0.186)	(0.327)	(0.092)
Reference date				
Year = 1999	0.315	0.415	0.310	0.483
	(0.465)	(0.493)	(0.463)	(0.500)
Year = 2000	0.332	0.360	0.327	0.316
	(0.471)	(0.480)	(0.469)	(0.465)
Year = 2001	0.353	0.224	0.362	0.201
	(0.478)	(0.417)	(0.481)	(0.400)
Month = January	0.072	0.059	0.068	0.051
	(0.259)	(0.235)	(0.252)	(0.220)
Month = February	0.082	0.056	0.085	0.058
	(0.274)	(0.230)	(0.279)	(0.233)

Table A.2: Continued

	Men		Women	
	NPC	PC	NPC	PC
Month = March	0.083	0.091	0.079	0.094
	(0.277)	(0.288)	(0.269)	(0.292)
Month = April	0.077	0.100	0.076	0.084
	(0.267)	(0.300)	(0.266)	(0.277)
Month = May	0.087	0.075	0.082	0.079
	(0.282)	(0.263)	(0.274)	(0.269)
Month = June	0.087	0.107	0.108	0.090
	(0.282)	(0.309)	(0.310)	(0.287)
Month = July	0.086	0.057	0.075	0.047
	(0.281)	(0.231)	(0.263)	(0.212)
Month = August	0.080	0.048	0.076	0.049
	(0.272)	(0.213)	(0.265)	(0.216)
Month = September	0.082	0.103	0.079	0.080
	(0.275)	(0.304)	(0.269)	(0.272)
Month = October	0.087	0.072	0.095	0.043
	(0.282)	(0.258)	(0.294)	(0.203)
Month = November	0.083	0.080	0.069	0.042
	(0.275)	(0.272)	(0.253)	(0.202)
Month = December	0.091	0.153	0.109	0.282
	(0.288)	(0.360)	(0.311)	(0.450)
n	24,821	8,531	14,880	4,363

Table A.3: OLS–estimates, one year *before* plant closure date

	Overall costs (incl. sick pay)	Overall costs (excl. sick pay)	Sickness leave	Consultations	Hospitalisations	Drugs	Days on sick leave
A. Men							
Days not employed	9.234***	0.793***	8.441***	0.014	0.735***	0.044	0.028***
	(1.280)	(0.144)	(1.207)	(0.018)	(0.122)	(0.045)	(0.005)
mean dep. var.	455.497	253.028	202.469	81.270	115.658	56.100	11.464
s.d. dep. var.	2918.971	723.886	2662.438	124.490	574.983	305.577	20.243
n	33,352	33,352	33,352	33,352	33,352	33,352	33,352
R^2	0.037	0.041	0.031	0.125	0.019	0.019	0.094
B. Women							
Days not employed	4.597***	0.943***	3.654***	0.067*	0.670***	0.206*	0.026***
	(0.897)	(0.196)	(0.775)	(0.026)	(0.132)	(0.102)	(0.005)
mean dep. var.	469.108	377.100	92.008	155.789	136.051	85.260	10.373
s.d. dep. var.	1725.697	818.573	1314.157	172.825	574.091	411.819	18.947
n	19,243	19,243	19,243	19,243	19,243	19,243	19,243
R^2	0.036	0.044	0.026	0.132	0.017	0.015	0.073
Control variables	✓	✓	✓	✓	✓	✓	✓
Industry dummies	✓	✓	✓	✓	✓	✓	✓
Regional dummies	✓	✓	✓	✓	✓	✓	✓
Time dummies	✓	✓	✓	✓	✓	✓	✓

Notes: *, **, *** denotes statistical significance on the 5%, 1% and 0.1% level, respectively. Robust standard errors (clustered by firm) in parentheses. The dependent variable in column 1 is overall health costs of a worker during the year before the plant closure (reference) date, overall costs excluding sick pay in column 2, various subcategories of health costs in columns 3 to 6, and days on sick leave in column 7 (see table ??). Days not employed measures the number of days not in employment during the year prior to plant closure (prior to the reference date). Included control variables are age, tenure, wage, firm size, and their squares. All control variables are measured at the last date before plant closure (before the reference date). There are 9 industry dummies, 28 regional dummies, and 15 time dummies (year and month).

CHAPTER

THREE

SUBJECTIVE EVALUATIONS OF WAGE INEQUALITY & THE DEMAND FOR REDISTRIBUTION

"Lack of money is the root of all evil."

George Bernard Shaw (1856–1950), Irish writer

3.1 Introduction

One of the primary objectives of the modern welfare state is the redistribution of income and wealth, either on grounds of supporting certain minimum standards of living for the less privileged (by providing social welfare, for example), or explicitly stated with the goal of reducing existing inequalities per se, which often serves as the rationale for progressive taxation (Barr, 1992; Boadway and Keen, 2000). It does not cause surprise then that all OECD countries spend huge amounts, both in absolute and relative terms, on redistribution (see, for example: OECD, 2007; Oxley *et al.*, 1997). In 1996, for example, the U.S. government spent about 21% of its total budget on social security and welfare; the corresponding figure for Germany is considerably higher and amounts to about 40% (Hindriks and Myles, 2006, p.56). In fact, overall expenditure on redistribution is arguably even higher than these figures suggest, since the provision of public goods is also financed by taxes and public goods often also have an implicit redistributive flavor (if, for example, poor(er) individuals disproportionally often take up publicly provided services). Thus, the question naturally arises of why so much redistribution takes place, at least in the western welfare states.[1]

[1]Besides, news about (sometimes) spectacularly high salaries of some few top managers more frequently find their way into the media and sometimes lead to heated, even fierce, debates in public and

Further, it is a commonly hold view in economics that the extent to which redistribution takes place is determined by voting, and is thus ultimately shaped by the preferences over fairness and redistribution of the electorate itself (see, among others: Boadway and Keen, 2000; Borck, 2007; Hindriks and Myles, 2006). In fact, inspired by the well–documented differences in the distribution of earnings (e.g. Gottschalk and Smeeding, 1997) and differences in the impact of redistributive measures (Kenworthy and Pontusson, 2005; Milanovic, 2000; Oxley *et al.*, 1997) between countries, recent theoretical work has pushed forward the idea that the amount of redistribution may essentially be linked to attitudes about distributive justice (Alesina and Angeletos, 2005) or to the perception of whether income and earnings are due to luck or effort (Bénabou and Tirole, 2006). In fact, as has been pointed out Bénabou and Tirole (2006), it is difficult to think about the huge differences between countries with respect to inequality and redistributive policy *without* any reference to differences in such beliefs. Empirically though, it remains to be shown that the observed differences in the amount and efficacy of redistribution is linked to corresponding differences in the demand for redistribution. Although this chapter will not directly address the question of differences between countries, it provides a simple conceptual framework that can easily be applied to such questions.

However, much less is known about the forces that shape the demand for redistribution in the first place, although by now a couple of empirical studies on the issue exists (see the discussion in section 3.2). Further, we almost completely lack empirical evidence on the hypothesized link between the demand for redistribution on the one hand and political outcomes on the other hand. It thus also remains to be shown that this is actually the case, that is, it remains to be shown empirically that there actually is a link between the demand for redistribution and political outcomes via voting behavior. Mainly for this reason, Switzerland is chosen for the empirical analysis, because we expect a clear link between the individual demand for redistribution and voting behavior, not the least because the political parties in Switzerland clearly position themselves regarding redistributive issues (and they have to do this quite often, since there are about ten votings over referenda and initiatives a year at the federal level alone, not counting the votes at the level of cantons or communities – although, obviously, not every voting is about redistribution. Also, because of its federal structure, both taxes and expenditures are to a significant part decided on small–level political units in Switzerland (i.e. on the level of cantons and communities), giving rise to tax competition within Switzerland and thereby accentuating this line of argument (Feld, 2000). Second, the Swiss labor market

politics. However, the public discussion has to date had a very limited focus and thus it does not provide us with a full picture of how overall wage inequality is perceived or of what kind of wage distribution is regarded as fair.

is very similar to the U.S. labor market and thus the results of this chapter may, with respect to this dimension, be compared to the large bulk of existing empirical studies from the U.S. (e.g. Nickell *et al.*, 2005)[2]

The primary aim of this chapter is to elaborate empirically on the these issues, relying on survey data from the International Social Survey Program (ISSP, 1999). First, I propose a simple empirical framework for measuring individual evaluations of wage inequality, both with respect to the perception of the factual as well as to the evaluation of the desired distribution of wages. These two subjective inequality measures (i.e. the discrepancy between them) are then used for measuring the demand for redistribution at the individual level. This conceptualization explicitly recognizes that individuals might differ both in their perception of the factual distribution and in their belief about the distribution they judge as legitimate. Demand for redistribution, conceptualized in this way, can thus only arise if the perceived level of inequality does not correspond with the desired level of inequality. Second, using this framework, I will explore the 'anatomy' of redistribution. That is, I will empirically explore whether the demand is primarily driven by the bottom or the top end of the distribution (or both, eventually) and whether is is driven by the perception of the actual state of the world or by the belief about how the world ideally should look like ('world' in this context means wage inequality). Third, I will study the importance of various factors in explaining the observed variation in the support for redistribution. Specifically, I will explore the question of whether and to what extent these differences can be attributed to either self–interest, to perceptions of and social norms over distributive justice, or to both of them. Fourth and finally, I will explore the empirical link between the demand for redistribution on the one hand and stated preferences over political parties on the other hand and thereby provide indirect empirical evidence on the hypothesized link between beliefs and political outcomes.

The main empirical findings of this chapter are the following. First, there is considerable support for the redistribution of wages (that is, support for at least some equalization of wages), resulting from the fact that the desired inequality in wages is on average considerably lower than the perceived inequality. At the same time though, people basically accept rather large differences in wages between different occupations (which, obviously, differ in their prerequisites, associated responsibilities, required educational attainment, and so on). Second, self–interest, perceptions of how wages are determined, and social norms with respect to distributive justice are important in explaining differences in the individual support for redistribution (a result in line with

[2]Besides, Switzerland seems to be an exceptional case regarding the evolution of income inequality, at least with respect to the incomes at the very top. See Dell *et al.* (2005) and Dell (2005) for a discussion of the income and wealth distribution in Switzerland over the past century.

existing empirical studies). There is thus no evidence for an exact match between the position an individual holds (e.g. the expected gain or loss from redisributive measures which is associated with this position) and the norms and beliefs regarding distributive justice of this person. Third, there is some (although rather weak) empirical evidence on the link between the demand for redistribution and stated party preference.

The rest of this chapter is organized as follows. Section 3.2 to follow discusses some relevant background literature, mainly focusing on the empirical literature concerned with behavioral motives for redistribution. Section 3.3 shortly describes the data source and its main strengths and weaknesses, but the main emphasis of the section is on subjective (i.e. individual) wage estimates for different occupations. These data will be key for the empirical framework used in the rest of the chapter. Section 3.4 then goes on to discuss how subjective evaluations of wage inequality and the demand for redistribution will be conceptualized and measured. Results are presented in sections (sections 3.5–3.7). In section 3.5, I will first present some empirical results describing the demand for redistribution. Section 3.6 then presents several simple regression models, where the importance of various determinants of the demand for redistribution is considered. Further, the analysis looks along the dimension of perception versus desire and the along the dimension of top versus bottom of the wage distribution. Empirical evidence on the link between the demand for redistribution on the one hand and party preference on the other hand is discussed in section 3.7. Section 3.8 concludes.

3.2 Related Literature

In very simple terms, support for redistribution might derive from either selfish or altruistic motives. First, an individual might support redistribution because she benefits from redistribution herself in the end (or, to be precise, her benefits are higher than the costs incurred by redistribution). As will be discussed below, several different arguments might induce selfish redistribution. On the other hand, redistribution might arise from the belief in a just world or likewise considerations about fairness and distributive justice. That is, an individual may support redistribution because she wants someone else to benefit (or suffer, potentially) from such policies, without profiting herself from these policies.

3.2.1 Selfish Redistribution

The 'classic' economic view on redistribution postulates that the demand for redistribution is directly linked to an individuals' position within the income distribution (as discussed by Meltzer and Richard, 1981; Roberts, 1977; Romer, 1975).[3] Important for the empirical analysis to follow in this chapter is that these models predict that (relatively) poor individuals will favor redistribution because they will gain disproportionately from redistribution, and (relatively) rich will oppose redistribution because they will have to pay disproportionately for redistributive policies.[4] We should thus, at the individual level, expect income to be an important predictor of the demand for redistribution, and we should expect the more demand for redistribution, the lower one's income. Moreover, individuals at the top end of the distribution should oppose any amount of redistribution. Existing empirical studies though consistently show that income per se is not a very strong predictor of the support for redistribution (see, specifically, Corneo and Grüner, 2002; Fong, 2001), quite surprisingly and contrary to the predictions of these theoretical models. The empirical results of this chapter, as will be discussed later, are in line with the previous empirical literature on the same subject.

Income Mobility

One proximate explanation for this finding might be the existence of, or at least the belief in, income mobility (see, for example, Bénabou and Ok, 2001 on the effect of (the belief in) prospective upward mobility on the demand for redistribution). The possibility of upward–mobility as well as the potential risk of downward–mobility might mitigate the relation between income and the demand for redistribution, in such a way that one's expectations about future income at least partially determine the support for redistributive measures (Alesina and La Ferrara, 2005). This might, for example, explain the observation that there is no uniform support for income redistribution among the poor, even if they would immediately gain from such policies (Fong, 2006). On the other hand, the risk (or fear, respectively) of downward mobility might lead some rich individuals to support income redistribution as a way of income insurance, if they are expecting their income to fall in the future (Piketty, 1995; Ravallion and Lokshin, 2000). Rich individuals might thus vote for redistribution in order to insure themselves against negative income shocks. As pointed out by Fong (2006), although the theoretical

[3]Chapter appendix 3.A makes the argument explicit in terms of a very simple model.

[4]At the aggregate level, one would expect higher inequality in factor income (i.e. income before taxes and transfers) to be related with higher redistribution by the state. Milanovic (2000), for example, though finds somewhat mixed empirical evidence in support of the argument. One possible explanation is that political participation is itself an increasing function of income, thus shifting more political power to the rich individuals (see Bénabou, 2000). Kenworthy and Pontusson (2005) in fact show that voter turnout is related to the amount of redistribution. Alternatively, redistribution might not always go from rich to poor but rather the other way around. A leading example in this regard is the public financing of (primarily higher) education since, as pointed out by (Fernandez and Rogerson, 1995), children from wealthier families tend to have a higher probability of attaining higher educational degrees.

argument linking prospective upward mobility and the demand for redistribution is very appealing by itself, the empirical support it has found so far is quite shaky.

Absolute versus Relative Income

As several authors have pointed out, well–being might not only depend on absolute income but also (or perhaps even more so) on relative income, i.e. on income relative to some reference point (e.g. Clark and Oswald, 1996; van Praag *et al.*, 2003). A similar argument might naturally apply to the demand for redistribution: Support for redistribution might be driven by the extent to which an individual thinks that his own pay is higher (lower) than what he thinks would be appropriate or legitimate, from his very own point of view (the study by Corneo and Grüner (2002) supports this argument). It might even be the case that the absolute level of income per se is irrelevant with respect to the demand for redistribution (or at least, this effect might be mitigated by this evaluation about the appropriateness of one's own income), but that the relevant driving force is the extent to which one is satisfied (or not, respectively) with his own level of income. That is to say, an individual with low income might not support any redistribution as long as she thinks that she ultimately gets what she deserves.

3.2.2 Fairness and the Belief in a Just World

Recent research in economics has put forward ideas about the driving factors behind support for redistribution, which are in a way somewhat less naive (sociologically, at least). In fact, ample and robust empirical evidence has accumulated showing that, for one reason or the other, people do care about fairness and distributive justice – beyond the sole strive after maximizing their own well–being. Fong *et al.* (2005) provide a good overview over the relevant lines of argument, Kluegel and Smith (1981) discuss the same issue from a sociological point of view.

It seems safe to say that two of the most important concepts behind theories of distributive justice are the 'need principle' and the 'equity principle' (e.g. Konow, 2003), not being mutually exclusive. According to the need principle, resources should be allocated according to individual need. On the other hand, the 'equity principle' rests upon considerations of proportionality and responsibility (Roemer (1998) discusses the closely related concept of equality of opportunity). Shepelak and Alwin (1986) discuss related concepts from a sociological point of view.

Another important factor in explaining variation in the individual support for redistribution are perceptions about the factual determinants of resources (Bénabou and Tirole, 2006). That is, individuals who belief that mainly factors outside an individuals' control (e.g. ascribed factors like gender or ethnicity) are important for ascending in the income distribution are presumably more inclined in supporting (more) redistribution of incomes. On the other hand, individuals who strongly belief that individual effort and / or skills (e.g. aquired factors like education) are important for why people differ in their incomes, are less prone to support income redistribution. As pointed out by Bénabou and Tirole (2006), this belief might be so strong that individuals might even see an unfair reality as fair, in order that their perception does not conflict with their own beliefs.[5]

Of course, it is left to the empirical analysis whether the perception of the allocation mechanism and beliefs about which factors should be important in determining this allocation are swamped by the individual's endowments. But most empirical studies in fact find that *both* financial self–interest and social norms and beliefs are important in explaining the observed demand for redistribution.

Fairness as a Behavioral Motive

Up to date, there is ample experimental evidence showing that individuals are motivated by concerns about fairness and reciprocity. Fehr and Schmidt (2001) provide a recent and extensive survey of experimental evidence regarding fairness and reciprocity as behavioral motives. A slightly different perspective is given by Levitt and List (2007), stressing the fact that insights gained from experimental data do not, normally, carry directly over into the 'real world'.

First, in the so called ultimatum game two persons have to agree on how to split a certain amount of money. The first person makes a proposal of how to divide the money. The second person can then either accept or reject this proposal, without any possibility of action. In the case of acceptance, the money is split up between the two persons according to the proposal of the first person. In the case of rejection, neither of them receives any money at all. The 'standard' economic prediction is that the first person will propose the smallest possible amount of money to the other person, knowing that the second person will accept any positive amount of money offered to him. However, the robust experimental evidence clearly contradicts this prediction. As Fehr and Schmidt (2001, p.5) point out, "a robust result in the ultimatum game, across hundreds of experiments, is that proposals offering the responder less than 20 percent of the available surplus are rejected with probability 0.4 to 0.6". Further, since the

[5]The leading example probably is that in the United States only a minority of people beliefs that luck determines income (Alesina *et al.*, 2001). Analogously, a majority in the U.S. thinks that the poor are poor because they are lazy and don't work hard enough to get out of poverty. This beliefs though strongly contrast with the fact that social mobility in the U.S. is not exceptionally high (Solon, 2002).

probability of rejection decreases in the amount of money offered, these results suggest that proposals of too low an amount are rejected because they are viewed as unfair.

Second, in so called dictator games people more often than not propose a non–zero amount of money, although they could keep all the money for themselves without the threat of being punished for such behavior (as is possible in the ultimatum game). The typical finding of such experiments is that usually more than 60% of the individuals propose a positive amount of money – the mean transfer of money on average equal to about 20% of the total endowment (Levitt and List, 2007).

Further, the pattern of outcomes in these experiments seems to be quite sensitive to how the initial endowments are assigned, which supports arguments that postulate that evaluations of fairness depend on the perception of how resources are allocated in the real world. That is, an individual who perceives that the 'poor' themselves are to be blamed for being poor, might oppose redistribution, and vice versa. For example, Clark (1998) finds that it does make a difference whether the initial endowment in the experiment is determined randomly or by the relative performance in a knowledge quiz before the experiment (also see Hoffman *et al.* (1996) on the effect of framing).

Stated Preferences for Redistribution

Evidence from non–experimental (i.e. survey) data seems to be consistent with experimental evidence in general, but these studies also give some additional insights. The study by Corneo and Grüner (2002) shows that the support for redistribution is driven by selfish motives, but not exclusively so. Their empirical results also support the notion that people, at a very fundamental level, seem to share a belief in distributive justice (for whatever reason – something which can not be decided on by using survey data). Third, the study also shows that one's relative position within the income distribution is important besides the absolute level of income. Similar results are presented in Fong (2001), using survey data from the U.S. Specifically, her study also shows that perceived causes for low income are relevant in explaining the support for redistributive policies. Corneo and Fong (2006) go one step further and try to estimate the willingness–to–pay for distributive justice. Interestingly, the study shows that people do not differ with respect to the (monetary) value they put on distributive justice (although they differ in the causes they believe to be responsible for differences in endowments).

A number of studies has shown that there are large differences in mean attitudes towards redistribution between (groups of) countries (see, among many others, Svallfors (1997)). Closest to my own framework is certainly the recent study by Osberg and Smeeding (2006). This paper also shows huge variation in in the demand for redistribution, not only with respect to average attitudes, but to the distribution of attitudes in general. Interestingly, this study suggest that the United States are not really exceptional with respect to such attitudes.

3.3 Subjective Estimates of Occupational Wages

3.3.1 Data Source

The data that are used in this chapter come from the International Social Survey Program (ISSP), which is an international collaboration of several survey organizations aiming at annual cross–national survey collaborations focusing on different main topics (e.g. environment, work conditions).[6] In 1987, the program conducted its first survey focusing on issues of social inequality, with the largest part of the questions focusing on the perception of the income distribution and the factors explaining individual incomes, issues of distributive justice as well as the role of the government regarding the distribution of incomes. Two more survey projects on the same topic then followed in the years 1992 and 1999, and yet another survey on social inequality is planned for the year 2009. This chapter uses the Swiss survey data collected in the context of the third survey of the ISSP on 'social inequality' dating from 1999.[7]

3.3.2 Measurement Error in Survey Data

Most economists seem to have quite strong reservations using survey data (instead of administrative or experimental data), since it is known that typical survey data are almost always subject to high rates of non–response and presumably prone to considerable measurement error, to name but the two most important problems. The study by Duncan and Hill (1985), for example, uses administrative data to validate survey responses for the very same workers. They find measurement error of varying degree, ranging from small error with respect to annual earnings and tenure to very large error with respect to hours worked (which obviously leads to large error in hourly earnings, even if annual earnings are not prone to such error). Moreover, there are more subtle (and perhaps even more troubling) problems with survey data, a leading example being that people sometimes express attitudes without really knowing what the questions are

[6] The ISSP originally grew out of a collaboration between the ZUMA (Zentrum für Umfragen, Methoden, und Analysen) in Mannheim and the NORC (National Opinion Research Center) in Chicago. To date, 42 countries take part in the program. See the organization's homepage for further information and the history of the program (www.issp.org).

[7] See Stamm *et al.* (2003) for details regarding the collection of the data and an extensive descriptive discussion of these data. The data can be obtained from the Swiss Information and Data Archive Service for the Social Sciences (SIDOS) under study no. 6396.

about (Bertrand and Mullainathan, 2001). On the other hand, however, survey data typically include information which open up research questions which otherwise would not be accessible to empirical analysis at all.

For the purpose of this study though, as will become clear when the conceptual framework will be discussed, the focus is *explicitly* on subjective evaluations of different occupational wages. In fact, the whole analysis would not make any sense if all individuals exactly knew the wages of workers from other occupations because there would be no variation between individuals regarding these evaluations and subjective evaluations of wage inequality would collapse with objective inequality for each and every individual (see section 3.4 below for details). In a way then, the empirical part of this chapter tries to to make a virtue out of necessity.

3.3.3 Sample Selection

Sample selection, as is is typical for survey data, is primarily driven by missing data and thus basically by the selection of the variables used in the subsequent analysis. For reasons of comparability and consistency, all results to follow are confined to the same sample of observations consisting of these 581 observations providing full information.[8] Observations are kept in the sample only if their demand for redistribution could have been computed (see section 3.4 below) and if they could have been included in all statistical models to follow (see sections 3.5–3.7 to follow).

3.3.4 Estimates of Occupational Wages

One of the most interesting parts of the survey is a battery of questions about the wages of ten different occupations, including the respondent's own occupation.[9] Individuals were first asked to estimate what they thought to be the actual net wage (i.e. wage net of social security contributions, but before taxes and transfers) per month in Swiss francs of people working in these different occupations. Second, they were asked to estimate what they thought these occupations should – in their subjective view – earn net per month in Swiss francs. These two wage estimates will be referred to as *actual wages* (or *perceived wages*, respectively) and *just wages* (*desired wages*) in what follows. These statements allow examining individual differences between the perceived and the

[8]Section 3.D.2 in the appendix shows that the quantitative results are not strongly affected by sample selection due to missing data (and thus the qualitative results are not driven by sample selection anyway).

[9]See Kelley and Evans (1993) for an international comparison of similar data on occupational wages from an earlier survey of the ISSP. Stamm *et al.* (2003) provide a more extensive descriptive discussion of these data for the case of Switzerland.

appropriate level of compensation within different occupations and will further be used to construct subjective measures of inequality and the demand for redistribution (see section 3.4 below). Descriptive statistics for these individual wage estimates are given in

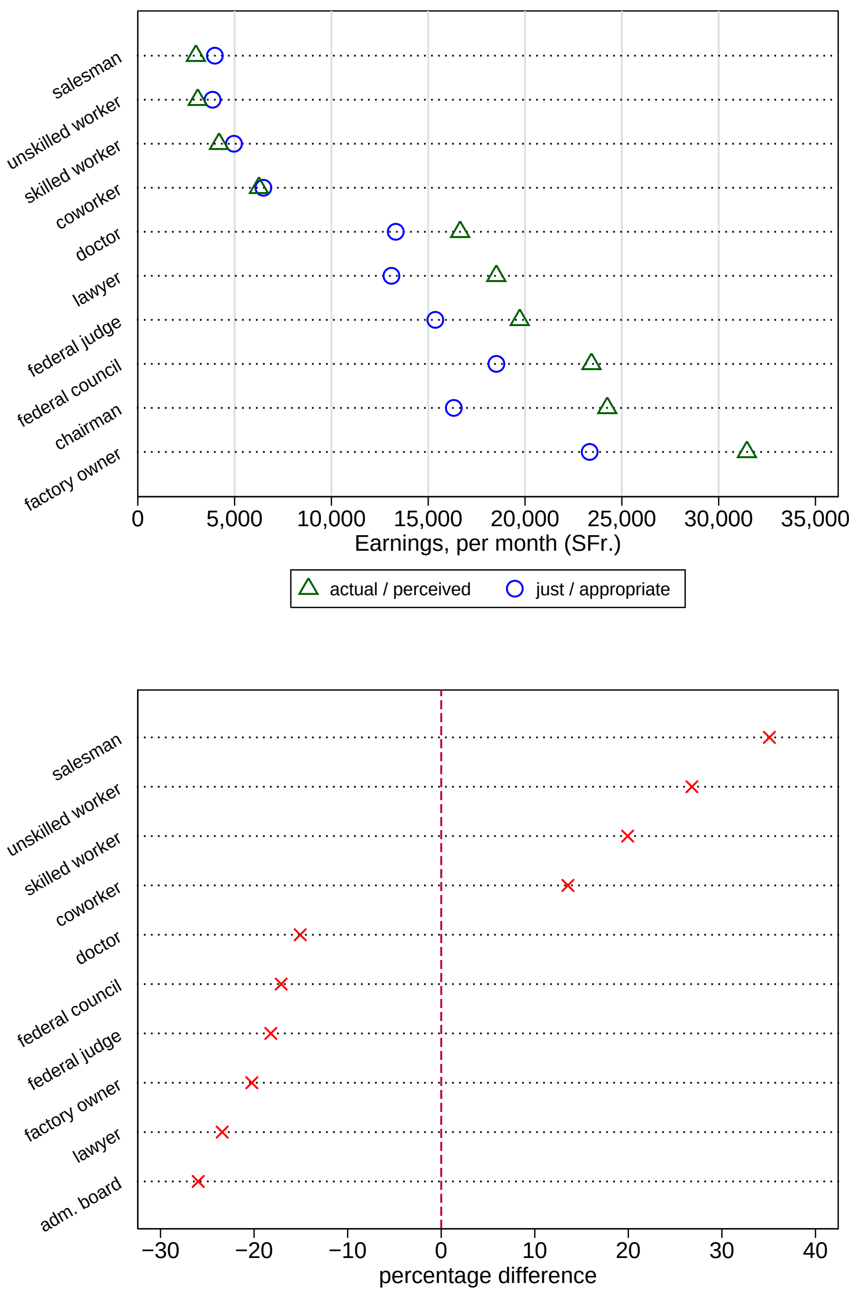
Table 3.1: Subjective estimates of occupational wages

	Actual wage	Just wage	Ratio
Low wage group:			
Salesman	3,004.15	3,977.03	1.35
	(573.05)	(753.52)	(0.26)
	[579]	[579]	[578]
Unskilled worker	3,110.07	3,856.08	1.26
	(605.63)	(708.63)	(0.23)
	[576]	[576]	[572]
Skilled worker	4,234.20	4,984.90	1.19
	(844.65)	(945.36)	(0.17)
	[579]	[576]	[575]
High wage group:			
Doctor	16,595.30	12,971.75	0.82
	(9,431.42)	(7,174.49)	(0.21)
	[575]	[577]	[572]
Lawyer	18,178.07	12,834.86	0.77
	(10,765.18)	(7,676.70)	(0.34)
	[570]	[568]	[566]
Federal judge	19,496.28	15,396.81	0.83
	(9,086.14)	(8,286.99)	(0.36)
	[565]	[565]	[563]
Member of the Swiss Federal Council	23,649.28	18,433.99	0.83
	(13,370.67)	(10,826.30)	(0.39)
	[554]	[562]	[550]
Owner of a factory	25,046.85	16,492.11	0.73
	(17,176.31)	(11,516.05)	(0.45)
	[555]	[558]	[549]
Member of the administrative board	32,623.05	24,304.17	0.79
	(21,126.94)	(16,962.14)	(0.28)
	[538]	[552]	[535]
Respondent's occupation			
Coworker	6,329.60	6,788.64	1.13
	(5,732.13)	(5,189.09)	(0.30)
	[581]	[581]	[581]

Notes: The table shows mean estimates of actual and just net monthly wages in Swiss francs (i.e. wages net of mandatory social security contributions, but before taxes and transfers). The third column shows the ratio of just over actual wage. Standard deviations are given in parentheses, the number of observations in brackets. The number of observations varies somewhat between different cells, since not all individuals gave estimates for all occupations. The maximum number of observations is 581 (which corresponds to the sample used in the analysis). Own calculations, based on ISSP (1999).

table 3.1. The first column shows mean estimates of actual wages, and the second column mean estimates of just wages for the ten different occupations. The third column shows the average of the ratio between just and actual wages. Focusing on mean estimates[10],

Figure 3.1: Estimates of occupational wages



Notes: The upper panel shows mean values of the actual / perceived (triangles) and just / desired (circles) wage estimates for the different occupations. The lower panel shows the mean percentage difference between desired and perceived earnings. Own calculations, based on ISSP (1999).

actual wages exhibit quite a wide range – from a low of about three thousand (wage of a shop assistant) to a high of more than 30 thousand Swiss francs (wage of a chairman of a large national company), thus giving a perceived range of actual wages of about ten on average. Further, note that there is a clear distinction between the occupations in terms of their actual wages – reflecting the distinction between three low–skilled and six high–skilled occupations. Consequently, since the average respondent lies somewhere in between, the actual wage of one's coworkers lies between the wages of these two groups (about 6,300 Swiss francs).

A first remarkable feature of the data is thus the fact that individuals seem to accept, at least on average, rather large differences in wages between different occupational groups, presumably reflecting considerations of proportionality and responsability (see section 3.2 above). In fact, focusing on individuals who gave estimates for all nine occupations, only two of them gave the same estimate for all occupations (they also gave the same estimate for the wage of their coworkers). This feature of the data is more easily seen in figure 3.1. Compared to actual wages, just wages exhibit a smaller but still a broad range – the highest average wage (owner of a factory) is still about six times as large as the lowest average wage (unskilled worker in a factory). It thus seems fair to say that people do accept wage differences due to differences in skill or educational level or differences in the degree of responsibility, although to a varying degree. At the same time though, most people are prone to equalize wages to a quantitatively important degree (see the last column in table 3.1 and the lower panel in figure 3.1). Average just wages are higher than average actual wages for those three occupations with the lowest estimated actual wages (i.e. salesman, unskilled and skilled worker). The reverse is true for the occupations with estimated high average actual wages. Specifically, as table 3.1 shows, there is one group of occupations (unskilled worker, skilled worker and salesman) for which there is a positive average difference between the two estimates and another group of occupations (all other professions, leaving out one's own occupation) for which this difference is negative on average. In what follows, the first group of professions will be referred to as the *low wage group* (or, interchangeably, *bottom group*) and the second is referred to as the *high wage group* (*top group*, respectively). As will be discussed in detail below, the measurement of subjective evaluations of wage inequality will be based on the disctinction between these two groups regarding the estimates of their actual and just wages.

[10]Figure E.1 in the chapter appendix provides a more detailed description of the distribution of these estimates, not exclusively focusing on averages.

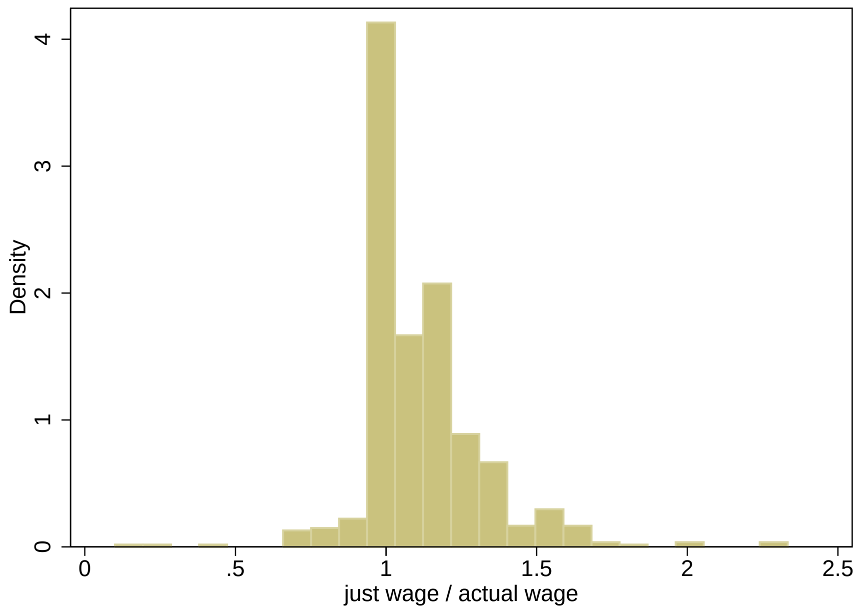
3.3.5 Coworkers' Wages

Estimates about the coworkers' wages are interesting on their own right, since these estimates can be used as an evaluation of the fairness of one's individual wage. Following Jasso (1999), I define individual i's justice evaluation of the wage of his coworkers (as a proxy for the justice evaluation his own wage) as:

$$je(i) = y(i)^{\star}_{\text{coworker}} / y(i)_{\text{coworker}} \tag{3.1}$$

where $y(i)^{\star}_{\text{coworker}}$ denotes i's estimate of the desired level of compensation, and $y(i)_{\text{coworker}}$ denotes the perceived wage level. Figure 3.2 shows the distribution of $je(i)$ in the sample.

Figure 3.2: Justice evaluation, wage of coworkers



Notes: The variable is defined in equation (3.1) in the text. Own calculations, based on ISSP (1999).

The main feature of the distribution clearly is the fact that most individuals tend to judge the wage of their coworkers (and thus, presumably, also their own wage) as appropriate (independent of the absolute level). In fact, about 38% of the individuals gave *exactly* the same estimate for the actual and the desired wage of their coworkers.[11] This result is somewhat surprising at first sight, since individuals were not given any restriction with respect to these estimates. Even more surprising perhaps is that (although very few) individuals state that their coworkers' wage should be lower than what they perceive it to

[11] Another quesion in the survey directly asked "Do you think that your wage corresponds to your effort and your skills?" More than 50% of the individuals in the sample think that their wage is appropriate in this sense.

actually be. Still, most respondents would like a higher wage than perceived, the average respondent would judge a wage about 13% higher than his actual wage as appropriate for him– or herself (for his coworkers, respectively). On the other hand, we can compare these estimates with the information the individuals provided about their own income, checking whether people give accurate estimates of their actual remuneration when asked about the wage of their coworkers. Focusing on individuals working full–time, their mean personal income is estimated to be about 5,900 Swiss francs – which is lower than their mean estimate of their coworkers' actual wage of about 6,500 Swiss francs.

3.3.6 Comparing Subjective Estimates and Actual Wages

At this point, it seems obvious to compare these subjective wage estimates with actual wages. The problem though is that, at least for some of these occupations, it is difficult to get reliable information about wages on the occupational level. One possibility though is computing average wages within different occupational groups using the ISCO (International Standard Classification of Occupations) code which is available in the data. To be specific at this point, actual wages per se are not observed, only incomes.[12] This procedure though does not give us reliable estimates of actual wages for the four top occupations (e.g. the chairman of a company) and is a bit shaky for the other wages too, because they are estimated using only a very small number of cases. Table 3.2 shows actual incomes from the ISSP data. The top panel (panel A) shows mean incomes by groups according to the major ISCO–classification (only for those groups which cover at least one occupation shown in table 3.1). The first major code certainly includes the two managerial occupations, and the second major code includes the two professional occupations. Salespersons are included in major code five. Although ambiguous, an unskilled worker certainly belongs to major code nine. A skilled worker again is ambiguous and might belong to either major code seven or eight.

The lower panel (panel B) of table 3.2 shows actual wages for the three occupations, which can unambiguously associated with an ISCO code on a more detailed level – something that is possible for salespersons, doctors and lawyers only. The average estimate for a salespersons' wage is surprisingly accurate (the average estimated wage of about 3,000 Swiss francs versus actual wage of about 3,200 Swiss francs). In comparison, the average estimates for the wages of a doctor and a lawyer are about twice as large as the actual wages estimated from the sample.

Below the line, the data seem to support four observations. First, individuals give surprisingly accurate wage estimates for the low–skilled occupations (i.e. salesperson,

[12]At least for the lower skilled occupations, this should though not make a huge difference. For the high skilled occupations, it presumably does so though.

Table 3.2: Actual income, by occupation (ISCO–code)

Occupational group	Actual income	n
A. ISCO–code (1 digit)		
1: Legislators, senior officials and managers	6,205.539	73
	(2,949.714)	
2: Professionals	5,948.748	199
	(3,077.408)	
5: Service workers and shop and market sales workers	3,219.752	162
	(2,470.527)	
7: Craft and related workers	4,511.771	135
	(2,130.938)	
8: Plant and machine operators and assemblers	4,205.814	50
	(2,721.436)	
9: Elementary occupations	3,560.370	29
	(1,798.359)	
B. ISCO–code (2 or 3 digits)		
222: Health professionals (excl. nursing)	8,550.815	10
	(5,045.615)	
242: Legal professionals	8,000.000	6
	(2,258.318)	
522: Salespersons and demonstrators	3,192.768	75
	(2,030.446)	

Notes: Table entries are monthly average incomes by respondents' ISCO–code. Standard deviations in parentheses. Own calculations, based on ISSP (1999).

skilled and unskilled worker in a factory). Second, people seem to have a tough time estimating the wages of high–skilled occupations, the exception being the wage of a member of the federal council and presumably also the wage of a federal judge. Third, the wages of the two professional occupations (i.e. doctor and lawyer) seem to be strongly overestimated. Fourth, statements about the estimated wages of the two managerial occupations must necessarily be quite shaky since it is very difficult to get accurate data about real salaries of these occupations – still it seems fair to say that even these estimates are not entirely made out of thin air.

3.3.7 A Little Twist

There is a little twist in using these data for evaluating the demand for redistribution because both actual and just wages are asked for before taxes and transfer payments (since net wage in Switzerland corresponds to gross wage net of mandatory social security contributions only).[13] The total amount of desired redistribution in occupational wages

[13]One could make the argument, although it might not be wholly convincing, that these data actually do cover the overall amount of desired redistribution – given that individuals implicitly make the relevant

would be given by comparing actual gross wages with desired net wages (i.e. wages after taxes/transfers), which would capture the total desired reduction (or increase, respectively) due to redistributive measures. That is, ideally we would like to evaluate $y^{\text{gross}}_{\text{actual}} - y^{\text{net}}_{\text{just}} = y^{\text{gross}}_{\text{actual}} - (1 - \tau(y))y^{\text{gross}}_{\text{just}}$, with $\tau(y)$ being the sum of the tax and transfer rate for some given gross income y. But, because we can only compare wages before taxes/transfers, we can only capture redistribution *on top* of the redistribution already implemented implicitly by the current system of taxes and transfers. That is, we can only evaluate $y^{\text{gross}}_{\text{actual}} - y^{\text{gross}}_{\text{just}}$. Under a flat tax rate (i.e. $\tau(y) = \tau$), for example, we will tend to underestimate the desired reduction in wages for the case of cutting high wages (the deviation is due to the fact that the taxation of the gross just wage is not taken into account). In the more realistic case of nonlinearities in the redistributive system (e.g. due to progressive taxation), it is more difficult to evaluate the magnitude of under– or overestimation.

3.4 Conceptual Framework: Subjective Measures of Inequality

The evaluation of whether the distribution of wages is judged as fair is ultimately in the eye of the beholder, since – as we already have discussed in section 3.2 above – the existing empirical evidence suggests that inequality aversion is relative to some fair reference point, which does not normally coincide with absolute equality. Moreover, both perceptions and beliefs may differ to a considerable degree between individuals.

It thus seems reasonable to conceptualize the demand for redistribution as the discrepancy between the perception of the actual distribution ('the actual state of the world') and the evaluation of the just distribution of wages ('the desired state of the world'), since people potentially can (and actually do) differ on both dimensions. As pointed out by Sen, "people's attitudes towards, or reactions to, actual income distributions can be significantly influenced by the correspondence – or lack thereof – between (1) their ideas of what is normatively tolerable, and (2) what they actually see in the society around them" (Sen, 2000, p.60). In a similar vein, Alesina and Angeletos (2005) model social injustice as the discrepancy between the actual and the just state of the world. This actually allows for the possibility that people differ in their support for redistriubution – even if they share the same perception of the factual inequality – if they differ in their evaluation of the just inequality.

On the other hand, individuals with different evaluatons of the just level of wage

comparison between gross and net wages.

inequality may differ in their support for redistribution because they do have different perceptions about the factual distribution of wages. Support for redistribution may arguably only arise if these two evaluations differ from each other, but differences between individuals might either be driven by differences in perception or by differences in the evaluation of what constitutes an appropriate compensation (or both).

3.4.1 Setting the Stage

We now turn to the discussion of a simple framework suited for describing subjective evaluations of wage distributions, wage inequality and the demand for redistribution.[14] A useful, and natural, starting point is the measurement of wage inequality in an objective sense. Let y_n be the vector of (ordered) individual wages for some random sample of size n (i.e. y_n corresponds to the wage distribution within the sample):

$$\begin{aligned} y_n &= \{y_1, \ldots, y_i, \ldots, y_n\} \\ \text{with} \quad & y_1 \leq y_2 \leq \ldots \leq y_{n-1} \leq y_n \end{aligned} \tag{3.2}$$

Principally, it is thus sufficient to observe y_n for measuring inequality in wages for a given sample, since inequality measures are some function of the vector of wages y_n (see Cowell, 2000, for example).[15]

Measuring subjective wage inequality is conceptually very simple and intuitive, in that we now allow the vector of wages y_n to depend on the evaluation of individual i, reflecting the fact that inequality (like beauty) ultimately lies in the eye of the beholder. In the simple symmetric case where each individual i estimates the wages of all individuals within the sample (including his own wage), we thus have:

$$y(i)_n = \{y(i)_1, \ldots, y(i)_j, \ldots, y(i)_n\} \tag{3.3}$$

Where $y(i)_j$ corresponds to i's evaluation of individual j's wage, yielding a matrix of wage estimates of size $(n \times n)$. Since in this case the whole distribution of wages is evaluated ("estimated"), we can compute individual measures of inequality and thus get a distribution of inequality measures over a sample of individuals. The obvious problem is now a practical one: Each individual i would have to estimate n (potentially) different

[14]Jasso has done related work in a series of papers (1978; 1980; 1999). More recently, Osberg and Smeeding (2006) have used an empirical framework similar to the one proposed here.

[15]For simplicity, I will leave any sampling issues aside here, focusing exclusively on conceptual issues. It is obvious though that under simple random sampling and as the sample size grows, the sample distribution y_n converges to the population wage distribution y. This in turn implies that sample inequality measures also converge to the corresponding population inequality measures.

wages – a task which surely is not feasible in practice for even moderate sample size n.

One possible solution is to reduce the number of different wage estimates one specific individual i has to make, i.e. to reduce the dimension along index j. Going back to objective inequality, note that we may approximate the distribution of wages, as given by equation (3.2), even if we only observe some average wages for a number of groups $k < n$ (again, ordered by within–group wage in ascending order):

$$y_k = \{(\overline{y}_1, f_1), \ldots, (\overline{y}_j, f_j), \ldots, (\overline{y}_k, f_k)\} \tag{3.4}$$

given we observe both the average within–group wages $\overline{y}_j$ as well as the appropriate weights f_j (i.e. f_j corresponds to the fraction of individuals in group j).[16] Knowledge of y_k allows us to approximate the individual wage distribution y_n, and thus allows us to approximate wage inequality in the sample without the need to observe the whole distribution of wages.

Again, measuring subjective evaluations of the wage distribution implies that we make the vector of wage estimates y_k dependent on i's evaluation:

$$y(i)_k = \{(\overline{y}(i)_1, f(i)_1), \ldots, (\overline{y}(i)_j, f(i)_j), \ldots, (\overline{y}(i)_k, f(i)_k)\} \tag{3.5}$$

Note that the reduction in the dimension of the number of estimates per individual k now introduces the problem that the weights of the different groups must also be estimated by individual i, if we want to make any statements about the wage distribution as a whole. Because, in the following, we want to exclusively focus on differences in the evaluation of wages and because people seem to be notoriously uncertain in estimating such shares, we can again simplify the problem by fixing the population weights between individuals:[17]

$$y(i)_k = \{(\overline{y}(i)_1, f_1), \ldots, (\overline{y}(i)_j, f_j), \ldots, (\overline{y}(i)_k, f_k)\} \tag{3.6}$$

We now have considerably simplified the original task (given by equation (3.3)), since now each individual i only has to estimate k (instead of n) different wages. In the most

[16]In the limit, as $k \to n$, we obviously have $y_k \to y_n$. For a given number of groups $k < n$, the approximation is better, the less within–group variation in wages there is. If there were no within–group variation in wages at all, then y_k is the same as y_n, even in the case where the number of groups k is smaller than the number of observations n.

[17]In terms of equations (3.3) and (3.5), fixing the weights may simply be understood as requiring that each individual estimates the wages for the same sample of individuals. From this point of view, is is quite natural to fix the population weights across individuals.

simple case, only two different groups are considered:

$$\begin{aligned} y(i)_2 &= \{(\overline{y}(i)_1, f_1), (\overline{y}(i)_2, f_2)\} \\ &= \{(\overline{y}(i)_1, f_1), (\overline{y}(i)_2, (1 - f_1))\} \end{aligned} \tag{3.7}$$

In this case, each individual i only has to estimate two different wages. Furthermore, since the sample weights must always sum up to 1, only one weight needs to be fixed explicitly. In the following section, I will discuss how the subjective estimates of occupational wages (see section 3.3.4) will be used to approximate the right hand side of equation (3.7).

3.4.2 Occupational Wage Estimates Reconsidered

The occupational wage estimates from the ISSP (1999) almost perfectly fit into the framework laid out in the preceding section. Moreover, these estimates may not only be used for computing individual estimates of the perceived (actual) distribution of wages, but also for computing the desired (just) distribution of wages. In the following, let $y(i)$ denote the vector of i's estimates of actual occupational wages:

$$\begin{aligned} y(i) = \{ & y(i)_{\text{Shop assistant}}, y(i)_{\text{Unskilled worker}}, y(i)_{\text{Skilled worker}}, y(i)_{\text{Doctor}}, y(i)_{\text{Lawyer}}, \\ & y(i)_{\text{Judge}}, y(i)_{\text{Minister}}, y(i)_{\text{Factory owner}}, y(i)_{\text{Chairman}}\} \end{aligned} \tag{3.8}$$

Analogously, let $y^{\star}(i)$ be the vector of i's evaluation of just occupational wages. However, note that these occupational wage estimates do not directly fit into the framework laid out above, because they do only cover a part of the distribution of occupations, and consequently only cover part of the wage distribution (i.e. we only observe a part of the right–hand side of equation (3.5)).

However, we may still use these data for approximating the average wages of larger groups (e.g. we will assume that some subsets of the nine occupations are each somehow representative of specific parts of the whole population wage distribution). As already discussed in section 3.3 above, there is a clear distinction between the nine occupations (excluding coworkers' wages) for which wage estimates were asked for. Specifically, let us group the nine occupations into two distinct sets:

$$\text{bottom} = \{\text{shop assistant}, \text{unskilled worker}, \text{skilled worker}\} \tag{3.9}$$

$$\text{top} = \{\text{doctor}, \text{lawyer}, \text{judge}, \text{minister}, \text{owner of a factory}, \text{chairman}\} \tag{3.10}$$

Where the first group of occupations (bottom) consists of the occupations with low actual wages (both relative to the occupations in the other group, but also in absolute terms) and desired wages above actual wages (or, in other words, the occupations in (3.9) have, on average, higher just than actual wages, and vice versa for the occupations in (3.10)).

We then use the occupational wages for estimating the mean wages of individuals at the top and the bottom of the wage distribution:[18]

$$\overline{y}(i)_{\text{bottom}} = \frac{\sum_j I(j \in \text{bottom}) y(i)_j}{\sum_j I(j \in \text{bottom})} \tag{3.11}$$

$$\overline{y}(i)_{\text{top}} = \frac{\sum_j I(j \in \text{top}) y(i)_j}{\sum_j I(j \in \text{top})} \tag{3.12}$$

Where $I(\ldots)$ denotes the indicator function. The analogous measures can be computed for the distribution of just wages, i.e. $\overline{y}^{\star}(i)_{\text{bottom}}$ and $\overline{y}^{\star}(i)_{\text{top}}$ if we replace the vector of actual wages $y(i)$ with the vector of desired occupational wages, $y^{\star}(i)$. Descriptive statistics for these variables are given in table 3.3.

By construction (i.e. due to the grouping of the nine occupations, see equations (3.11) and (3.12)), mean wage estimates of the bottom group are on average lower than mean wage estimates of the top group, both with respect to the actual and the desired wage distribution. Now, due to the specific choice of occupations, the two group averages describing the actual wage distribution seem to be somewhat off target – primarily due to the fact that only occupations from the two ends of the distribution were included in the wage estimates (but no 'typical' occupation from the 'middle', so to say).[19] For comparison, if we look at table 3.1 again, we see that the average estimate of coworkers' wage (about 6,300 Swiss francs) is considerably higher than the corresponding average estimate given in table 3.3 (about 5,400 Swiss francs).

[18] There are at least four different reasons for aggregating the occupational wage data. First, in any case do I have to make some additional assumptions about the frequencies of the different occupations in order to fit the framework laid out before. However, this is most easily and plausibly done for two broad groups only. This seems to be especially true because some of the occupations have obviously very low (or almost zero) frequency in the whole population (e.g. member of the Swiss Federal Council). Second, estimates for some specific occupations may be largely off the mark, but the average over several occupations may still give a reliable estimate of what an individual perceives to be the wage of a larger group. Third, the problem of missing data on the dependent variable can to some extent be mitigated as averaging over several occupations allows me to compute subjective inequality measures as long as an individual gave at least one wage estimate for each of the two sets of occupations (also see chapter appendix 3.D.2). Fourth, one might also argue that people often implicitly make this differentiation between the bottom and the top (rather between several occupational groups).

[19] To be precise, we do not expect correspondence between objective and subjective wage distributions; to the contrary, it would be surprising *not* to observe at least some difference between estimated and actual wage (incomes, respectively). Here, I am only pointing to the fact that the specific choice of occupations might generate *some* of the observed difference between the objective wage distribution and subjective evaluations of the wage distribution.

Table 3.3: Subjective evaluations of group–specific wages, wage inequality, and the demand for redistribution

	Mean	Standard deviation	Coefficient of variation
A. Actual (perceived) wages			
Low wage group, $\overline{y}_{\text{bottom}}$	$3,451.32$	552.33	0.16
High wage group, $\overline{y}_{\text{top}}$	$22,745.61$	$10,114.35$	0.44
Overall, $\overline{y}$	$5,380.75$	$1,183.85$	0.22
B. Just (desired) wages			
Low wage group, $\overline{y}^{\star}_{\text{bottom}}$	$4,273.78$	681.77	0.16
High wage group, $\overline{y}^{\star}_{\text{top}}$	$16,805.74$	$8,378.07$	0.50
Overall, $\overline{y}^{\star}$	$5,526.98$	$1,092.86$	0.20
C. Subjective inequality measures			
Actual inequality, gc	0.31	0.10	0.32
Just inequality, $gc^{\star}$	0.19	0.09	0.47
Demand for redistribution, dr	36.95	21.68	0.59

Notes: The definitions of the variables are given in the text. The number of observations equals 581 throughout. Own calculations, based on ISSP (1999).

Given individual estimates of the mean wages of the bottom and top of the wage distribution, there remains one piece of information before we have individual approximations of the wage distributions and thus can compute individual inequality measures. The remaining parameter is the population share of the bottom of the distribution f_{bottom} (or the population share of the top, respectively – see equation (3.7)). As already mentioned, we fix the two population shares between individuals, so that we only have to fix one specific value for f_{bottom}. One obvious choice is to estimate f_{bottom} from the occupational distribution in the sample at hand:

$$f_{\text{bottom}} = \frac{1}{n} \sum_{i=1}^{n} I(\text{ISCO}_i \in [3, 9]) \tag{3.13}$$

Whereas ISCO_i corresponds to the one–digit (main) ISCO–code of i's occupation. The top of the distribution thus only comprises ISCO–codes 1 (legislators, senior officials and managers) and 2 (professionals), all other occupations are subsumed into the bottom category. Note that, because the population is divided into two groups only, $f_{\text{top}} = (1 - f_{\text{bottom}})$. In the sample at hand, f_{bottom} evaluates to approximately 23%. Given the 'unusual' composition of occupations (some occupations in this group are exceptional both with respect to their wages as well as to the frequency within the overall occupational distribution) within the top group, I will correct this number downwards and "guesstimate" f_{bottom} as 10%.[20]

Leaving problems of missing data aside at this point (section 3.D.2 in the chapter appendix takes the problem of missing data up again), we thus now observe for each individual i the two triples:

$$y(i) = (\overline{y}(i)_{\text{bottom}}, \overline{y}(i)_{\text{top}}, f_{\text{bottom}}) \tag{3.14}$$

$$y^{\star}(i) = (\overline{y}^{\star}(i)_{\text{bottom}}, \overline{y}^{\star}(i)_{\text{top}}, f_{\text{bottom}}) \tag{3.15}$$

Where $y(i)$ now describes individual i's evaluation of the actual wage distribution and (corresponding to the 'actual state of the world'), analogously, $y^{\star}(i)$ describes individual i's evaluation of the desired wage distribution (the 'desired state of the world') – both of which exactly fit the conceptualization exemplified in equation (3.7) above for the case of two different groups only. Again, note that f_{bottom} is treated as a fixed parameter, as it does neither very between individuals not between the evaluation of the actual and the desired wage distribution.

3.4.3 Subjective Measures of Wage Inequality

Given the estimates for group–specific mean wages (equation (3.11) and (3.12), respectively) and the population weights of the groups (equation 3.13), is is now straightforward to construct an overall estimate of the population mean wage by appropriately weighting the wage estimates of the two groups:

$$\overline{y}(i) = \overline{y}(i)_{\text{bottom}} \cdot f_{\text{bottom}} + \overline{y}(i)_{\text{top}} \cdot f_{\text{top}} \tag{3.16}$$

Again, $\overline{y}^{\star}(i)$ provides an estimate of individual i's evaluation of the desired overall wage. Because the two weights are the same not only between individuals but also for the evaluation of the actual and the desired distribution, differences between $\overline{y}(i)$ and $\overline{y}^{\star}(i)$ must necessarily be due to differences in the underlying wage estimates. Descriptives for these overall mean wages are also given in table 3.3. One point noteworthy is that the average overall wage for the actual distribution is almost the same as the overall wage of the just wage distribution, although the group wages markedly differ (I will take up this point in section 3.4.4).

Furthermore, and more importantly, these three statistics are sufficient for computing

[20] In the year 2000, the fraction of people with a university degree in Switzerland has been 15.8% for people aged 25–64, which presumably still gives an upper bound for f_{bottom}. Section 3.D.1 in the chapter appendix takes up the issue of how the empirical results change when different assumptions are made regarding the two population weights.

the relative wage share of the bottom group:[21]

$$q(i)_{\text{bottom}} = \left(\frac{\overline{y}(i)_{\text{bottom}} \cdot f_{\text{bottom}}}{\overline{y}(i)_{\text{bottom}} \cdot f_{\text{bottom}} + \overline{y}(i)_{\text{top}} \cdot (1 - f_{\text{bottom}})}\right) \tag{3.17}$$

Given population and wage shares of the two groups, we are ready to compute inequality measures. One can easily show (see appendix 3.C) that the Gini coefficient is given by the following simple expression in the case of two different groups only:

$$gc(i) = f_{\text{bottom}} - q(i)_{\text{bottom}} \tag{3.18}$$

Since the population share f_{bottom} is the same for all individuals, all variation in the subjective Gini coefficient $gc(i)$ is due to variation in the evaluation of occupational wages.[22]. Equation (3.18) also directly shows that changing the estimates of the two population weights changes the subjective inequality measures, although it does not change the ranking of individuals with respect to these measures (this point is further discussed in chapter appendix 3.D.1). Now, we can also compute the Gini coefficient related to the desired wage distribution $gc^{\star}(i)$, simply by replacing actual wage estimates by just wage estimates (i.e. using the desired wage share instead of the actual wage share of the bottom group):

$$gc^{\star}(i) = f_{\text{bottom}} - q^{\star}(i)_{\text{bottom}} \tag{3.19}$$

We may then, at this point, define the demand for redistribution simply as the desired percentage reduction in the perceived level of wage inequality:

$$dr(i) = -100\% \cdot \left(\left(\frac{gc^{\star}(i)}{gc(i)}\right) - 1\right) \tag{3.20}$$

The redistribution measure $dr(i)$ combines the perceived and the desired level of wage inequality and fits our basic intuition about the demand for redistribution.[23] Demand for redistribution increases, ceteris–paribus, when the perceived wage inequality is high or when the desired level of wage inequality is low. At the same time, no matter how high or how low the perceived level of inequality is, redistribution is only supported if

[21] And, since the wage shares of the two groups have to sum up to 1, the wage share of the top group is given by $q(i)_{\text{top}} = 1 - q(i)_{\text{bottom}}$.

[22] Note that the Gini coefficient could actually be negative in this framework if the wage share of the bottom group would be larger than f_{bottom}, which is not ruled out a priori. Empirically though, this case is not observed at all. Also note that, in the case of two groups only, the ranking of individuals in their evaluation of wage inequality is unambiguous.

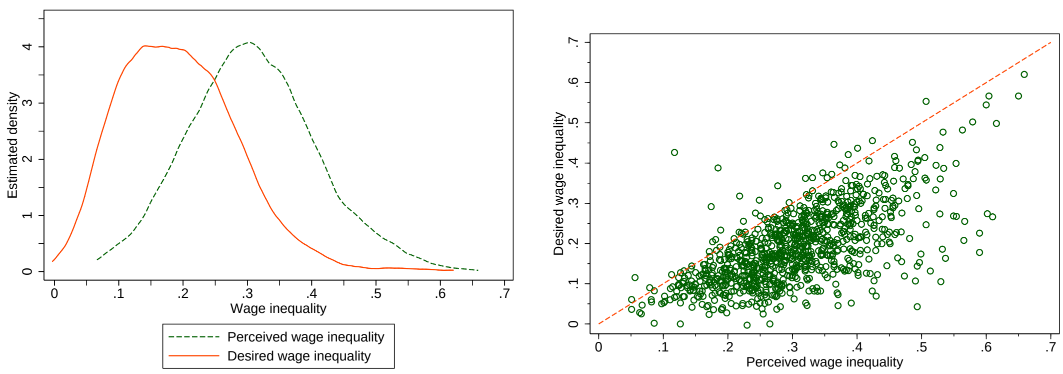
[23] Alesina and Angeletos (2005), for example, also model social injustice as the deviation of the actual state from some reference point.

there is any discrepancy between the perceived and the desired level of wage inequality.

This variable measures the extent to which people would like to decrease (or increase, eventually) the level of wage inequality, as a percentage of the perceived factual wage inequality. Descriptive statistics for these three variables measuring subjective wage inequality and the demand for redistribution are given in table 3.3, which shows descriptive statistics for the average estimates of the actual and the just wages of the *low wage group* and the *high wage group* respectively, as given by equations (3.11) and (3.12). Further, descriptives for the two subjective evaluations of the wage distribution and the demand for redistribution are also shown (equations (3.18), (3.19), and (3.20)).

As already discussed, the average overall actual wage is lower than the overall just wage for the low wage group. That is, the average individual would like to increase the mean wage of this group. On the other hand, people would on average like to decrease the mean wage of the high wage group. The average person would like to decrease the ratio of the two mean wages from about 6.5 to about 4. It is further interesting to note that the variation relative to the mean decreases slightly for the low–wage group and does increase for the high–wage group (in both cases, it is about as three times as high for the high– as for the low–wage group). Actual wage inequality is estimated as 0.30 on average. Interestingly, this number is not too far off actual estimates of the income inequality, which is estimated to be 0.30 for the sample at hand. The estimated actual wage inequality is higher than the just wage inequality, which is about 0.19. The mean of the demand for redistribution must therefore be positive, as one might have expected on a–priori grounds. The mean desired reduction in wage inequality is estimated to be about 37%, a considerable number (note though the large standard deviation). The

Figure 3.3: Subjective inequality measures, univariate and joint distribution



Notes: The figure on the left shows kernel density estimates using the Epanechnikov kernel function and the 'optimal' bandwith. Figure on the right: The x–axis shows the perceived wage inequality measure and the y–axis the desired wage inequality measure, as defined in equation (3.19) and (3.18), respectively. Own calculations, based on ISSP (1999).

investigate further, figure 3.3 shows both the density estimates for the two subjective gini coefficients and their joint distribution. The figure on the left shows the estimated univariate distributions of the two wage inequality measures. The comparison clearly shows both a shift of the distribution to the left as well as a change in shape. Perhaps more interestingly, the figure on the right shows that almost all individuals desire a lower level of wage inequality than what they actually perceive. By construction then, most people have a positive demand for redistribution, i.e. most people would favor a more equal distribution of wages.[24]

3.4.4 The Costs of Redistribution

Another interesting point in question is whether and to what extent people (implicitly) make redistributive statements which are budget–neutral, i.e. whether we can evaluate if the desired reduction in inequality is associated with any additional costs (in terms of the wage sum needed to adapt to the desired wage level of both the low– and the high–wage group) or whether people are aware that, for example, higher wages at the bottom of the distribution must somehow go hand in hand with lower wages at the top. The results considered thus far suggest that people are aware that wages cannot simply be set at will, and that an increase of the wage at the bottom of the distribution must be financed by eventually decreasing the wages at the top. Further scrutiny of the data does indeed suggest that, at least on average, people tend to balance increases at the bottom with decreases at the top.

We can again empirically approach the question of how much resources, in terms of the actual wage distribution, are needed in order to obtain the desired level of of wage inequality (in the following called 'costs of redistribution'):

$$cr(i) = \left(\frac{f_{\text{bottom}}(\overline{y}^{\star}(i)_{\text{bottom}} - \overline{y}(i)_{\text{bottom}}) + f_{\text{top}}(\overline{y}^{\star}(i)_{\text{top}} - \overline{y}(i)_{\text{top}})}{\overline{y}(i)} \right) \tag{3.21}$$

This measure can be used for evaluating whether and how much (or less, possibly) resources are needed in order to attain the desired level of wage inequality and thus can be used for evaluating whether people, at least implicitly, acknowledge that there is a constraint on the amount of redistribution possible. Note that $cr(i)$ need not be positive,

[24] The right–hand side of figure 3.3 also shows that, perhaps somewhat surprisingly, there are some individuals for whom the just wage inequality is higher than the perceived inequality, resulting in a negative demand for redistribution. Further scrutiny of these 39 observations though shows that the reason for their negative demand for redistribution is not that they want to redistribute from the bottom to the top. What actually happens here is that these individuals want to increase not only the wage of the low–wage group, but also the wage of the high–wage group (and the desired increase of the high–wage group is higher than the desired increase of the low–wage group).

although most individuals seem to have a desire to level up the wages at the bottom of the distribution, because at the same time most people would like to cut the wages at the top such that the second term in the nominator is most often negative. Which of the two terms dominates, however, is an empirical question. Table 3.4 shows various

Table 3.4: The costs of redistribution

Redistribution from (to)	Mean	Standard deviation	Coefficient of variation
A. In absolute terms			
Bottom group	822.46	507.91	0.62
Top group	$-5,939.87$	$6,852.69$	-1.15
Bottom group (weighted by f_{bottom})	740.22	457.12	0.62
Top group (weighted by f_{top})	-593.99	685.27	-1.15
Overall	146.23	736.35	5.04
B. In relative terms			
Bottom group	0.14	0.10	0.67
Top group	-0.10	0.10	-0.99
Overall, cr	0.04	0.13	3.26

Notes: See equation (3.21) in the text on how the variables are constructed. Note that panel A only refers to the numerator of equation (3.21). Own calculations, based on ISSP (1999).

statistics for the absolute and relative extent of redistribution of wages, following from the difference between actual and just wage estimates. On average, the mean wage of the low wage group is increased by only about 740 Swiss francs, whereas the mean wage of the high wage group is decreased by almost six thousand Swiss francs. Taking the population weights into account and re–expressing the extent of redistribution in terms of the overall actual wage, one gets the result that the additional wage sum (about 14% of the overall actual wage) needed to raise the wage of the low wage group is almost offset by the wage sum freed up by decreasing the wages at the top (about 10% of the overall actual wage). Taken literally, these results imply that the implementation of the desired redistribution of the average individual would incur additional costs of about 4% of the overall wage sum.

3.5 The Anatomy of Redistribution

3.5.1 Additional Empirical Moments

Since we observe several occupational wage estimates for most individuals, we may also compute some additional moments describing more specific features of individual evaluations of the wage distribution on top of the overall wages and wage inequality.

Again, let $y(i)$ denote individual i's estimate of actual occupational wages, and let $y^\star(i)$ denote i's evaluation of just occupational wages (see equation (3.8)). I will now argue that there are two important dimensions, which specifically merit further empirical scrutiny. First, we will explore, with respect to the desired distribution, whether differences in the demand for redistribution are driven by differences regarding ethical bounds on bottom and top wages. Second, I will take a look at redistribution along the bottom–top dimension. That is, I will look at whether redistribution is mainly driven by desired wage changes at the top or at the bottom of the distribution. In order to get more specific, let's first define some additional measures.

First, we define the ethical floor $f(i)$ as the minimum wage regarded as fair, relative to the just average wage (in order to make these statements comparable across individuals, since average wage estimates differ between individuals):

$$f(i) = \left(\frac{\min(y^\star(i))}{\overline{y}^\star(i)} \right) \tag{3.22}$$

Analogously, we define the ethical ceiling $c(i)$ as the maximum wage regarded as fair, again relative to the average just wage:

$$c(i) = \left(\frac{\max(y^\star(i))}{\overline{y}^\star(i)} \right) \tag{3.23}$$

These two measures describe, in a way, the lower and upper bound of the desired wage distribution for each individual.

A second interesting aspect describes the equalization of wages, which can occur both by either increasing wages at the bottom or by reducing the wages at the top of the distribution. We define as a measure for the wish of leveling up $u(i)$ the wages at the bottom of the distribution:

$$u(i) = \left(\frac{\min(y^\star(i))/\overline{y}^\star(i)}{\min(y(i))/\overline{y}(i)} \right) \tag{3.24}$$

Analogously, we define $d(i)$ as a measure for the wish for leveling down wages at the very top of the distribution as:

$$d(i) = \left(\frac{\max(y^\star(i))/\overline{y}(i)^\star}{\max(y(i))/\overline{y}(i)} \right) \tag{3.25}$$

Table 3.5 shows descriptive statistics for these additional four measures, along with the overall wage estimates (which we already know to be more or less the same on average, see table 3.3 again). Panel A of table 3.5 shows moments describing the perceived

Table 3.5: Empirical moments describing subjective wage distributions

	Mean	Standard deviation	Coefficient of variation
A. Perceived wage distribution			
Overall wage, $\overline{y}$	5,380.75	1,183.85	0.22
Maximum, $\max(y(i))$	38,766.27	21,034.52	0.54
Minimum, $\min(y(i))$	2,843.72	515.53	0.18
Ratio, $\max(y(i))/\min(y(i))$	14.03	7.98	0.57
B. Desired wage distribution			
Overall wage, $\overline{y}^\star$	5,526.98	1,092.86	0.20
Maximum, $\max(y^\star(i))$	27,416.01	17,410.63	0.64
Minimum, $\min(y^\star(i))$	3,693.98	683.41	0.19
Ratio, $\max(y^\star(i))/\min(y^\star(i))$	7.67	5.15	0.67
C. Additional moments			
Ethical floor, $f(i)$	0.68	0.12	0.17
Ethical ceiling, $c(i)$	4.73	2.34	0.49
Level up, $u(i)$	1.28	0.27	0.21
Level down, $d(i)$	0.71	0.27	0.38

Notes: The exact definitions of the variables are given in the text. The number of observations equals 581 throughout. Own calculations, based on ISSP (1999).

distribution of wages across the different occupations. Not surprisingly, evaluations of the highest wage have the highest variation across individuals, much higher than both the evaluations of the overall wage and the lowest wage within the group of occupations. The ratio of highest to lowest wage (within individuals) is on average about 14, which again underlines the fact that people perceive large variation in wage (presumably higher variation than there actually is, as discussed in section 3.3.6).

Panel B shows the same moments as the first panel, but now with respect to the desired distribution of wages. Although there is no large difference of the desired overall wage to the perceived overall wage, both the lowest and the highest wage significantly differ from the respective moments in panel A. The highest desired wage is about 10,000 Swiss francs lower than the corresponding number for the perceived distribution. At the same time, the desired lowest wage is about 800 Swiss francs higher than the perceived lowest wage. On average, the ratio of highest to lowest desired wage is only about 7.5 (half of the corresponding number regarding the actual distribution of wages).

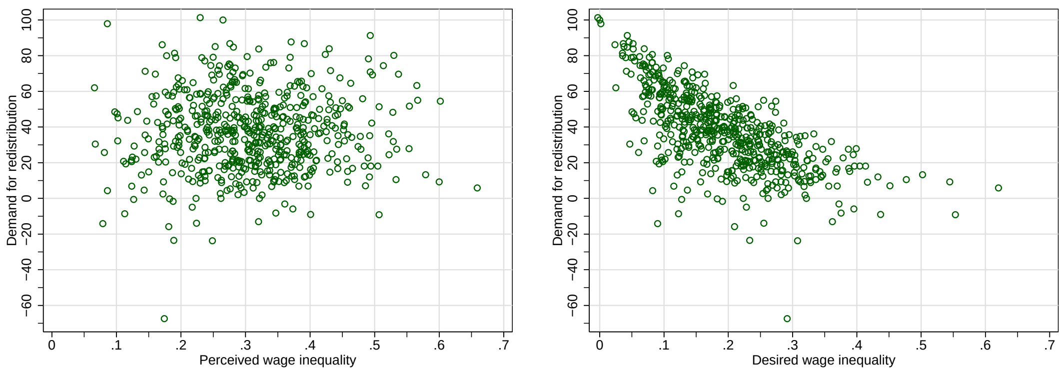
Finally, panel C shows the four additional moments of primary interest here. The first two measures, again, show that people accept differences in wages. The average individual though puts a lower limit on wages of about 70% the overall wage and an upper limit of about 4.8 times the overall wage. Moreover, attitudes are much more similar regarding the lower tail of the wage distribution (comparing either the standard deviation or the coefficient of variation). The other two variables show that, on the one hand, the average individual would like to push up the wage at the bottom by about one third. On the other hand, the desired change regarding the upper tail of the distribution mirrors the desired change at the lower tail. On average, top wages of about 70% their perceived level are judged as appropriate.

3.5.2 The Mechanics Behind Redistribution

Having established that most people do actually favor some redistribution (i.e. desire at least some reduction in wage inequality) and before turning to the question of whom wants to redistribute, we ask the related question of which feature of the wage distribution is most responsible for this outcome.

First, one wonders whether the demand for redistribution is primarily driven by the perceived wage inequality *or* by the desired level of wage inequality. This essentially boils down to the question of whether people have different perceptions about the real wage distribution *or* different beliefs about what a fair distribution of wages would ideally look like (or, eventually, both). Figure 3.4 shows the relation between the demand for

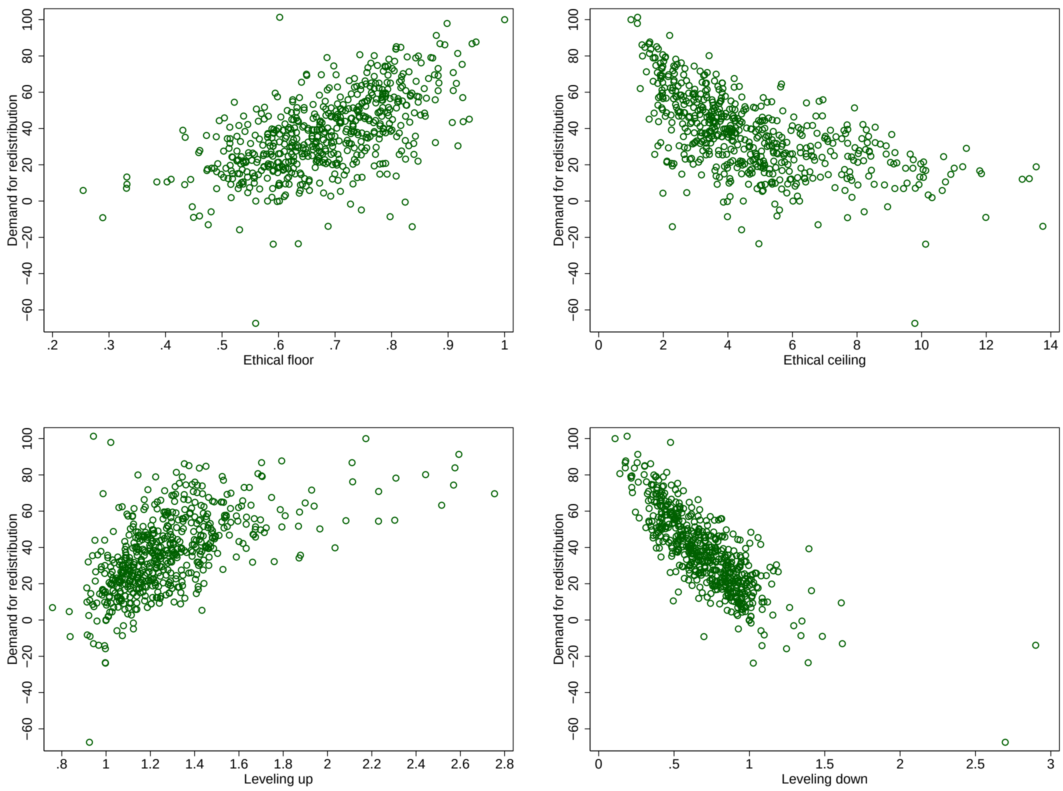
Figure 3.4: Redistribution versus subjective wage inequality



Notes: The y–axis shows the demand for redistribution as defined by equation (3.20), the x–axis shows the perceived wage inequality as defined in equation (3.18) (figure on the left) and the just wage inequality as defined in equation (3.19) (figure on the right). Number of observations equals 581 in both figures. Own calculations, based on ISSP (1999).

wage redistribution and the two subjective gini coefficients. Interestingly, there is almost no correlation ($r = 0.0677$) between the perception of the actual wage distribution and the demand for redistribution. On the other hand, there is a clear negative correlation ($r = -0.6302$) between the evaluation of the desired wage inequality and the demand for redistribution. These two figures together do suggest that the desire for redistribution of wages is primarily driven by the evaluation of the just distribution and to a lesser extent by the perception of the actual inequality.[25]

Figure 3.5: Demand for redistribution versus additional moments



Notes: The y–axis shows the demand for redistribution, as given by equation (3.20) in the text. The x–axis shows the variables defined in equations (3.22)–(3.25). The number of observations equals 581 in all figures. Own calculations, based on ISSP (1999).

Second, one can ask whether the wish for redistribution is driven by desired changes of the lower or the upper tail of the wage distribution (as has been discussed in section 3.5.1 above). Because most people gave several (i.e. more than two) estimates of occupational wages, we can explore this issue in more detail. Specifically, we now look at the four additional moments mentioned at the beginning of this section. I will primarily look at

[25]Figure E.1 in the chapter appendix plots some of the moments describing subjective wage distributions over deciles of the redistribution measure. The top panel shows moments of the actual distribution and the lower panel shows moments of the just distribution. The top panel shows that the extent of redistribution is by and large independent of the perception of the actual wage distribution – even when considering the distinction between the upper and the lower tail of the distribution (which, of course, is consistent with figure 3.4 above). The lower panel of figure E.1 on the other hand shows that the desired wage distribution is driving the observed variation in the amount of redistribution desired. More specifically, the figure shows that high levels of redistribution are first and foremost associated with different beliefs regarding the upper tail of the wage distribution.

the four moments defined in section 3.5.1. Figure 3.5 shows a simple scatterplot of each of these moments against the demand for redistribution.[26] The relations all look as one might expect. A higher ethical floor (ceiling) goes hand in hand with a higher (lower) demand for redistribution. On the other hand, the higher the desired leveling up of the wage at the bottom of the distribution, the higher the demand for redistribution (and vice versa, as expected, with respect to the leveling down of the wages at the very top).

We can actually be somewhat more precise regarding the 'anatomy' behind the demand for redistribution, and simply estimate some simple regression models of the following form:

$$dr_i \;=\; m_i'\beta + \epsilon_i \tag{3.26}$$

Where dr_i corresponds to the demand for redistribution, and m_i is a vector of variables describing various moments of the subjective wage distribution of individual i, either along the dimensions of perception–versus–belief or along the dimension of top–versus–bottom of the wage distribution. These regression models give the possibility to look at the simultaneous correlations among the different measures. There is no need to put any assumptions on the error term ϵ at this point, because we use these regression models *explicitly* as a descriptive tool describing the mechanics behind the demand for redistribution only. Also note that we can run these regressions only because the dependent variable is not directly a linear function of any of these moments (we must though be careful not to include too many regressors in order to maintain some useful ceteris–paribus interpretation of the individual regressors).

The resulting estimates are given in table 3.6. The first two models look at the perceived and the desired distribution separately. Column 1 essentially shows that, conditional on the overall wage estimate, only the perception of the lower bound of the distribution systematically correlates with the demand for redistribution, but not the perceived upper bound.[27] Also note that the predictive value of this model is essentially non–existent (which though is in line with figure 3.4). Column 2 shows that the demand for redistribution is empirically linked to the desired wage distribution both through the bottom and the top of the corresponding wage distribution (again conditional on the respective overall wage estimate, also note that in this case the R–squared jumps to 0.425).[28] Next, the model in column 3 simultaneously estimates the effects of all four

[26]Figure E.2 in the chapter appendix shows the full matrix of bivariate scatterplots.

[27]The perceived floor and ceiling are defined exactly as the ethical floor and ceiling (as given by equations (3.22) and (3.23), only that instead of $y^\star(i)$ the vector of actual wages $y(i)$ is used.

[28]Normally, of course, one would not care too much about the R–squared. But here interest lies in the mechanical, so to say, relations between the variables and thus in this specific context the R–squared

Table 3.6: The 'mechanics' behind redistribution

	Demand for redistribution				
Mean			36.954		
Standard deviation			21.679		
Perceived floor	−33.220***		−79.272***		
	(11.861)		(8.068)		
Perceived ceiling	0.001		2.927***		
	(0.434)		(0.346)		
Ethical floor		70.733***	103.550***		44.734***
		(9.529)	(8.364)		(7.707)
Ethical ceiling		−3.430***	−5.509***		−0.586*
		(0.466)	(0.521)		(0.316)
Level up				27.663***	21.151***
				(2.996)	(2.443)
Level down				−50.317***	−43.117***
				(5.017)	(4.554)
Mean overall wage, actual distribution	−0.004***		−0.001	−0.004***	−0.002**
	(0.001)		(0.001)	(0.001)	(0.001)
Mean overall wage, just distribution		0.002**	−0.000	0.000	0.001
		(0.001)	(0.001)	(0.001)	(0.001)
Constant	76.436***	−3.346	21.675***	58.278***	17.275*
	(10.783)	(9.976)	(8.267)	(5.815)	(9.592)
n	581	581	581	581	581
R^2	0.029	0.425	0.710	0.720	0.761
Adjusted R^2	0.024	0.422	0.707	0.718	0.758
p–value (F–statistic)	0.000	0.000	0.000	0.000	0.000

Notes: Robust standard errors in parentheses. All variables as defined in the text. Own calculations, based on ISSP (1999).

variables. This model in way only reflects the construction of the dependent variable, of course. But still, this model shows that all for moments at the same time determine the observed variation in the demand for redistribution. Interestingly, in this case the overall wages are not statistically different from zero any more. And although in this model the two moments describing the bottom and top of the actual wage distribution do have a practically and statistically significant effect, an equal (hypothetical) change in one of the two moments describing the desired distribution still induces a larger change in the demand for redistribution. Next, column 4 regresses the demand for redistribution on the two variables describing the equalization of wage (via increase (decrease) of either the wage at the very bottom and top). The estimates suggest that the variation in the demand for redistribution is more tightly correlated with variation in the desire for equalization via decreasing the top wages than with variation in the desire for leveling up wage at the other end of the distribution. Finally, model 5 simultaneously estimates the effects of the two moments describing the desired distribution and the two moments describing the desired changes at both ends of the wage distribution (this model obviously more or less corresponds to the model given in column 3, with a slightly different focus though). Again, all four parameters (of interest) are statistically significant and have the expected sign. This last model shows that the wages at the top are correlated with the demand for redistribution primarily via the channel of leveling the top wages down (the coefficient of the 'ethical ceiling' is practically zero). At the same time, the wish for leveling bottom wages up correlates with the amount of desired redistribution; the coefficient of the ethical floor though still remains large (both in practical and statistical terms).

The bottom line is that the extent of redistribution is a function of both the perceived and the desired distribution of wages. Interestingly, the absolute level of the wage distribution is only important regarding the perception, but not the desire. Differently put, the results of table 3.6 suggest that the variation in the demand for redistribution is simultaneously driven by both the perception of how the distribution of wage actually looks like and by the desired distribution. Viewed from another angle, both leveling up of bottom wage and leveling down of top wages drive the variation in the amount of redistribution desired.

3.6 Who Wants to Redistribute?

This section addresses the second question of main interest, namely: Which factors help explain the variation in the desired extent of redistribution. First, we look at various

delivers useful information about the correlational structure of the data.

determinants of the demand for redistribution pushed forward by previous literature, with main focus on financial self–interest and norms about distributive justice as well as perceptions of the causes generating inequality. Second, we will explore in some detail the channels by which different variables influence the amount of redistribution demanded by looking along the dimension of perceived versus desired inequality and along the dimension of top versus bottom of the wage distribution (analogous the the moments described in section 3.5).

3.6.1 Financial Self–Interest, Distributive Justice, and the Perception of Wage Determinants

As discussed in section 3.2 above, there are several potential explanations for the observed variation in the demand for redistribution between individuals. The empirical analysis tries to approximate as closely as possible the potential explanatory factors discussed there. In order to assess the importance of different factors in explaining the demand for redistribution, several simple linear regression models of the following form are estimated:

$$dr_i \quad = \quad \text{sinterest}_i'\alpha + \text{norms}_i'\beta + \text{beliefs}_i'\gamma + x_i'\ \delta + \epsilon_i \tag{3.27}$$

Where dr_i is the redistribution measure as defined in equation (3.20), sinterest_i is a (column) vector containing several variables capturing the effect of 'selfish' motives (that is, the vector contains relative personal income, the justice evaluation variable, and an index of mobility)[29]. norms_i is a also a column vector, containing two variables approximating the two primary norms about distributive justice (e.g. the principle of need and the principle of effort, as discussed in section 3.2). The vector beliefs_i contains two variables which are meant to capture the perception of individuals of how resources are actually allocated in the real–world (the two variables are meant to describe whether the respondent thinks that either acquired skills or ascribed skills are actually the driving forces behind the determination of wages). x_i is a vector of additional control variables.[30] α, β, γ and δ are the corresponding vectors of parameters. ϵ_i is an error term assumed to be i.i.d. and mean independent of all regressors. Now, although our aim is not (or

[29] The variables are defined in chapter appendix 3.B.

[30] The following variables are included as additional controls: Age in years, age squared, education (highest attained level, in years), female dummy, foreign–born dummy, residence in the german speaking part of Switzerland, living in an urban area, two dummy variables for unemployment and nonemployment (employment as reference category), dummy indicating whether a person is employed full–time (versus part–time), perception of conflicts, standard international occupational prestige scale and political orientation. See appendix 3.B for the exact definitions of these variables.

cannot be, given the structure of the data) estimating any causal parameter, we still have to think about unobserved factors contained in the error and which potentially bias our estimates of primary interest (which are α, β and γ). We thus adopt a somewhat agnostic control function approach, in that we try to control for as many potentially important factors as possible in order to minimize biased estimation of the main parameters. By additionally including age, gender and education (among others) as regressors, I hope to mitigate as far as possible confounding by both unobserved factors (e.g. risk aversion) and potentially only weak proxies (e.g. mobility).

Several regression models are estimated, the results of which are given in table 3.7. Since my interest mainly focuses on the two groups of variables describing (i) financial self–interest with respect to redistribution and (ii) social norms about distributive justice and perceptions of the wage generating process. I run separate regressions in which either only one group is included as regressors or either both are included. All models are estimated with and without additional control variables given by the vector x.

The first model simply regresses the redistribution measures on relative personal income. The parameter estimate has the expected negative sign. Note though that the implied quantitative effect is not very large (i.e. doubling the personal income reduces the demand for redistribution by about 8.5 percentage points), and that the predictive power of income by itself is rather low (R–squared of 0.054), which is consistent with previous empirical studies (Fong, 2001). More importantly still, note that the predicted demand for redistribution is positive over the whole range of observed income, even for individuals with the highest income. Next, the fairness evaluation of one's own wage and a mobility variable are added to the regression. The coefficient of income changes somewhat, but still is significantly negative. The fairness evaluation of one's income and the mobility index both have the expected sign. The higher the perception of financial underreward, the higher is the demand for redistribution (holding actual income constant). Personal mobility has, as expected, a negative effect, although this effect is only significant after controlling for additional variables. Including additional control variables does not change the parameter estimates of the variables of main interest by much, but the model fit is significantly increased.

In a second step, various variables describing social norms about distributive justice and perceptions of which factors are important in explaining differences in pay are included as regressors. Again, all variables enter with the expected sign. Individuals who stronlgy belief that income should correspond to needs more strongly favour redistribution of earnings. On the other hand do people with a strong belief in the principle of effort show significant less support for redistribution. The two variables capturing the effect of different perceptions of which factors are important in determining the allocation

Table 3.7: Main results, demand for redistribution

	Demand for redistribution					
Mean	36.954					
Standard deviation	21.679					
Relative income	−8.755***	−8.204***	−7.948***	−7.471***	−6.731***	−7.035***
	(1.395)	(1.402)	(1.445)	(1.889)	(1.453)	(1.920)
Justice evaluation		6.242***	6.185***	6.253***	5.086**	5.479**
		(2.335)	(2.340)	(2.388)	(2.166)	(2.228)
Mobility			−0.575	−1.176**	−0.289	−0.904
			(0.586)	(0.587)	(0.583)	(0.592)
Needs					2.190**	1.568
					(0.983)	(1.041)
Effort					−8.260***	−5.348***
					(1.809)	(1.966)
Ascribed skills					2.053*	2.288*
					(1.230)	(1.193)
Aquired skills					−3.696***	−2.794**
					(1.387)	(1.412)
Control variables included?	No	No	No	Yes	No	Yes
n	581	581	581	581	581	581
Adjusted R^2	0.053	0.059	0.059	0.141	0.110	0.164
p–value (F–statistic)	0.000	0.000	0.000	0.000	0.000	0.000

Notes: *, **, *** denotes statistical significance on the 10%, 5%, and 1% level, respectively. Robust standard errors in parentheses. Additional control variables included are: Occupational prestige scale (SIOPS), dummy for full–time employment, employment status, age (and its square), female dummy, education (years), dummy for urban residency, dummy for living in the German–speaking part of Switzerland, dummy for foreign citizenship, political self–assessment (on a simple left–right scale), and a scale describing the perception of conlicts. Also see chapter appendix 3.B and table E.2 in the appendix showing full regression results. Own calculations, based on ISSP (1999).

of resources also show the expeced sign and are significant, even after including several control variables. Individuals who perceive ascribed (acquired) skills to be important in determining pay, tend to have lower (higher) demand for redistribution.

Including all variables at once does not change the results in any important way, although some variables get statistically insignificant. This supports the view that neither financial self–interest nor perceptions and social norms are sufficient for explaining the overall variation in the support of redistribution. In particular, the results presented here do not support the view that perceptions and social norms are unimportant, once self–interest in redistribution is controlled for. On the contrary, both sets of variables are important in explaining differences in the support for redistribution.

3.6.2 Perceived versus Desired Level of Wage Inequality

Table 3.8 presents separate regression results for both the demand for redistribution (the models are exactly the same as the model given by equation (3.27), only the dependent variable is the perceived or the desired level of wage inequality, respectively). That is, we run the following regression models:

$$gc_i = \text{sinterest}_i'\alpha + \text{norms}_i'\beta + \text{beliefs}_i'\gamma + x_i'\ \delta + \epsilon_i \tag{3.28}$$

Where gc_i corresponds to either the perceived inequality in wages (gc_i) or the desired inequality in wages ($gc_i^\star$).

Results are given in the first two columns of table 3.8. There are some interesting results concerning the explanation of the subjective income inequalities. First, it seems to be much more difficult to explain differences in the perception of the income inequality than differences in the desired level of earnings inequality (the R–squared from the explaining the desired level of wage inequality is more than twice as large as the R–squared from the regression explaining the perceived level of wage inequality). Thus perceptions may be more idiosyncratic tha evalutions of what is seen as appropriate; at least given the considered set of variables. The only variables enterning with a statistically significant coefficient are the two variables about the perception of which factors actually are important in getting ahead. The perception of the allocation mechanism is relevant a factor in explaining differences in the perception of the factual income inequality, but not for explaining the desired level of wage inequality. On the other hand, almost all variables are statistically significant and have the expected sign in explaining the just level of earnings inequality (with the exception of the two perceptual variables and the mobility index). Interestingly then, there are not systematic differences in the perception of wage inequality between individuals with different incomes – but they do differ

Table 3.8: Main results, additional moments

	Wage inequality		Ethical	Ethical	Level up	Level down
	Actual	Just	floor	ceiling		
Mean	0.308	0.193	0.680	4.733	1.282	0.711
Standard deviation	0.099	0.092	0.116	2.341	0.268	0.271
Relative income	0.010	0.030***	−0.029***	0.408**	−0.035	0.030
	(0.009)	(0.008)	(0.011)	(0.200)	(0.023)	(0.025)
Justice evaluation	−0.014	−0.027**	0.026	−0.649***	0.002	−0.043*
	(0.012)	(0.013)	(0.017)	(0.249)	(0.030)	(0.022)
Mobility	−0.000	0.003	−0.002	0.046	−0.008	0.014*
	(0.003)	(0.002)	(0.003)	(0.062)	(0.008)	(0.008)
Needs	−0.004	−0.008**	0.011**	−0.119	0.017	−0.013
	(0.005)	(0.004)	(0.005)	(0.104)	(0.012)	(0.014)
Effort	−0.002	0.020**	−0.030***	0.279	−0.099***	0.062**
	(0.010)	(0.008)	(0.011)	(0.202)	(0.031)	(0.024)
Ascribed skills	0.014**	0.000	−0.004	−0.030	0.034**	−0.026*
	(0.006)	(0.005)	(0.006)	(0.116)	(0.016)	(0.014)
Aquired skills	−0.015**	−0.001	−0.001	0.076	−0.039**	0.030
	(0.007)	(0.006)	(0.008)	(0.159)	(0.017)	(0.019)
Control variables included?	Yes	Yes	Yes	Yes	Yes	Yes
n	581	581	581	581	581	581
Adjusted R^2	0.079	0.166	0.144	0.105	0.103	0.086
p–value (F–statistic)	0.000	0.000	0.000	0.000	0.000	0.000

Notes: *, **, *** denotes statistical significance on the 10%, 5%, and 1% level, respectively. Robust standard errors in parentheses. Also see the notes of table 3.7. Own calculations, based on ISSP (1999).

with respect to their evaluation of what distribution is seen as appropriate.

3.6.3 Additional Moments

As already discussed in section 3.7, additional information can be gained by looking at the top and the bottom of the subjective wage distributions and the desire for equalizing wages via changes at the bottom and the top of the distribution. Table 3.8 thus also shows results for models of the form:

$$m_i = \text{sinterest}_i'\alpha + \text{norms}_i'\beta + \text{beliefs}_i'\gamma + x_i'\ \delta + \epsilon_i \tag{3.29}$$

Here, m_i is one of the four moments discussed in section 3.5.1 (again, besides the dependent variable, equation (3.29) exactly mirrors the model from equation (3.27)).

The results of these four models are also shown in table 3.8 (columns 3–6). These models also yield some interesting additional insights. First, the evaluation of the wages at the bottom of the distribution seems to be somewhat more structured than the wages at the top. Looking at the effect of income, table 3.8 reveals that income primarily affects the desired distribution of wages, and has the strongest effect on the spread at the top of the distribution. The same is true for the fairness evaluation of one's own income. Interestingly, norms with respect to distributive justice in this case only have a significant effect on the bottom of the desired wage distribution (note though that the point estimates are larger for the top of the distribution, but the estimates are not statistically different from zero). Finally, and consistent with results already discussed, the two variables describing the perception of how wages are formed in reality affect redistribution mainly through their effect on the top of the perceived distribution.

3.7 Linking Beliefs to Outcomes

As discussed in section 3.2 at the beginning, it is of main interest whether subjective beliefs about the fairness of the wage distribution and the demand for redistribution may translate into political outcomes and thus may indirectly be linked to the actual (i.e. observed) amount of redistribution. Switzerland is an interesting case in this respect, because there potentially is a direct link between norms about inequality and political outcomes through voting in general and party preference in particular. Moreover, the political parties in Switzerland explicitly position themselves publicly regarding issues which very often have a redistributive aspect.

Table 3.9 presents some simple models of stated party preference, where the dependent variable is the stated preference for some political party and our main interest lies on the effect of the demand for redistribution, or alternatively some other moment of subjective evaluation of the wage distribution.[31] Again, we are not making claims about causal relationships here, but nonetheless it presumably is more plausible that beliefs and norms are determinants of party preference, rather than the other way around. I will thus try assess empirically the link between (stated) party preference and the various measures describing different aspects of both the perceived and desired wage distribution. To keep things as simple as possible (econometrically), I simply run a series of linear probability models of the form:[32]

$$p_i = m_i'\zeta + \text{sinterest}_i'\alpha + \text{norms}_i'\beta + \text{beliefs}_i'\gamma + x_i'\delta + \epsilon_i \tag{3.30}$$

Party preference is mapped on a binary indicator function for each of the five political parties. m_i is a vector containing a (sub)set of the subjective measures already discussed in the preceding sections. sinterest_i, norms_i, beliefs_i and x_i are the same (column) vectors of control variables as used in equation (3.27) above. ζ is the parameter vector of main interest in this case, capturing the effect of subjective wage evaluation on the stated party preference.[33]

However, the preceding section has shown the the various moments describing subjective evaluations of the wage distribution are clearly correlated with at least some of the control variables. I thus also run the same regressions on a restricted set of control variables. Specifically I estimate the parameters of the following models:

$$p_i = m_i'\zeta' + x_i'\delta + \epsilon_i \tag{3.31}$$

Table 3.9 shows (parts of the) regression results for stated party preference and the demand for redistribution.[34] Panel A shows results for a model as given by equation (3.30), that is with the full set of control variables. Panel B shows the corresponding estimates when only the restricted set of controls is used (see equation (3.31)). First

[31] Only the five largest (with respect to voting shares) parties are considered here. These are the liberal–democratic party (FDP, "Freisinnig Demokratische Partei"), the christian democrats (CVP, "Christlichdemokratische Volkspartei"), the right of center conservative Swiss people's party (SVP, "Schweizerische Volkspartei"), the social–democratic party (SPS, "Sozialdemokratische Partei der Schweiz"), and the left of center green party (GPS, "Grüne Partei der Schweiz").

[32] One might of course use a more sophisticated statistical model like a multinomial logit. For the sake of simplicity with respect to the interpretation of the results, I will nonetheless rely on simple OLS estimation.

[33] I also estimated a model in which the dependent variable is an indicator taking on the value one, if *any* party preference is stated. In this case, no statistical effect whatsoever is found and thus I do not report these results here.

[34] Full regression results and results including other moments of subjective wage distributions are available upon request.

Table 3.9: Stated party preference and demand for redistribution

	FDP	CVP	SVP	SPS	GPS
Mean	0.197	0.128	0.199	0.331	0.041
Standard deviation	0.398	0.335	0.400	0.471	0.199
A. Full set of control variables					
Demand for redistribution	−0.004***	0.000	0.002	0.002	0.001**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)
Adjusted R^2	0.174	−0.002	0.166	0.245	0.066
p–value (F–statistic)	0.000	0.104	0.000	0.000	0.706
B. Restricted set of control variables					
Demand for redistribution	−0.006***	−0.000	0.001	0.004***	0.001**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)
Adjusted R^2	0.120	0.002	0.123	0.091	0.036
p–value (F–statistic)	0.000	0.010	0.000	0.000	0.290
n	366	366	366	366	366

Notes: *, **, *** denotes statistical significance on the 10%, 5%, and 1% level, respectively. Robust standard errors in parentheses. Also see the notes of table 3.7 and footnote 31. Own calculations, based on ISSP (1999).

Table 3.10: Stated party preference and subjective inequality measures

	FDP	CVP	SVP	SPS	GPS
Mean	0.197	0.128	0.199	0.331	0.041
Standard deviation	0.398	0.335	0.400	0.471	0.199
A. Full set of control variables					
Perceived inequality	−0.919***	−0.149	0.643**	0.607**	0.108
	(0.268)	(0.256)	(0.306)	(0.297)	(0.128)
Desired inequality	1.021***	−0.091	−0.404	−0.529	−0.269**
	(0.308)	(0.258)	(0.337)	(0.337)	(0.134)
Adjusted R^2	0.159	−0.002	0.170	0.246	0.062
p–value (F–statistic)	0.000	0.141	0.000	0.000	0.763
B. Restricted set of control variables					
Perceived inequality	−1.188***	−0.203	0.643**	1.052***	0.111
	(0.276)	(0.251)	(0.306)	(0.341)	(0.136)
Desired inequality	1.408***	−0.036	−0.404	−1.071***	−0.321**
	(0.305)	(0.248)	(0.337)	(0.378)	(0.144)
Adjusted R^2	0.097	0.003	0.170	0.091	0.032
p–value (F–statistic)	0.000	0.023	0.000	0.000	0.361
n	366	366	366	366	366

Notes: *, **, *** denotes statistical significance on the 10%, 5%, and 1% level, respectively. Robust standard errors in parentheses. Also see the notes of table 3.7 and footnote 31. Own calculations, based on ISSP (1999).

note that the difference between panel A and B is as expected, in that in the models with the full set of control variables a significant part of the effect of m on party preference is absorbed by the controls. Otherwise, the point estimates are as one would expect them to be. The demand for redistribution has a negative effect on the probability of stating preference over the liberal party (FDP), but a positive effect on stating preference for one of the two left of center parties (SPS, GPS). Interestingly, the amount of redistribution desired also has a positive effect on stating preference for the right of center party (SVP) – although this corresponds with what is known about voter turnout of the different parties.

Table 3.10 shows basically the same models as table 3.9, only that the vector m_i now includes the two subjective inequality measures instead of the demand for redistribution. Again, the top panel shows regression results using the full set of controls, whereas in the regressions shown in the lower panel only the restricted set of controls is used. Again, there are no significant effects regarding the preference for the christian–democratic party whatsoever. Party preference for one of the other four parties is however correlated with either the perceived or the desired wage inequality (or both) and consistent with the results from table 3.9. First, preference for the liberal–democrats is significantly shaped by both the perceived wage inequality and the desired wage inequality. That is, individuals who perceive wage inequality to be high whose desired wage inequality is small tend to state less preference for the FDP. In the case of the three remaining political parties (that is, SVP, SPS and GPS), the estimated parameters of the two moments switch sign.

In sum, the results of this section are somewhat mixed. On the one hand, there clearly is *some* evidence on the link between subjective evaluations and perceptions of the wage distribution. Moreover, the results presented in this section are consistent with what is known about party affiliation in Switzerland. At the same time though, the empirical evidence is rather weak (with respect to the quantitative effects).

3.8 Conclusions

This chapter has presented a simple and intuitive empirical conceptual framework for describing subjective evaluations of wage distributions, both with respect to the perceived as well as to the desired distribution of wages. This conceptualization also naturally leads to a simple measure of the demand for redistribution as the discrepancy between an individual's perceived and desired distribution of wages.

The main empirical findings of this chapter are the following. *First*, these is considerable support for at least some redistribution, which in most cases comes about as a combined effect of a desired increase of the wages at the bottom of the distribution and a decrease of the wages at the very top. Still, most individuals do accept large differences in wages, presumably reflecting the fact that people think that occupations differ in their educational requirements, responsibilities, and so forth. *Second*, there is some empirical evidence that selfish motives partially explain differences in the amount of redistribution desired, i.e. individuals with higher wages demand less redistribution (but still they also demand on average some positive amount of redistribution). Personal income alone though is a remarkable weak predictor of the support of redistribution of wages. Norms about distributive justice and beliefs about how wages are determined in reality also explain some of the observed variation in redistribution, even conditional on selfish motives. This result is in line with the existing empirical literature using non–experimental data. *Third*, there is some empirical evidence on the hypothesized link between the demand for redistribution and political outcomes (via party preference), in that all effects with respect to stated party preference have the expected sign, i.e. individuals with higher demand for redistribution tend to vote for left of center parties (and vice versa).

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3.A A Very Simple Model

The exposition here essentially follows Hindriks and Myles (2006). Suppose that individuals derive utility from income y and from a lump–sum public good g provided by the government. The public good is financed by a linear income tax with tax rate τ:

$$u_i(y,g) = (1-\tau)y_i + b(g) \tag{A.1}$$

Where we assume that $b'(g) > 0$ and $b''(g) < 0$, such that the marginal utility from the public good is positive, but decreasing in g. Further assuming that the government budget is balanced, the government's budget restriction is simply given by (the number of individuals is normalized to 1):

$$g = \tau \cdot \mu \tag{A.2}$$

Where μ corresponds to the average income in the population. Using (A.2), i's utility from the public good can be written as:

$$u_i(g) = \left(1 - \frac{g}{\mu}\right) y_i + b(g) \tag{A.3}$$

Thus individual i's optimal (i.e. desired) level of the public good is given by the first order condition:

$$\frac{\partial u_i(g)}{\partial g} = -\frac{y_i}{\mu} + b'(g) = 0 \tag{A.4}$$

The optimal level of the public good thus depends on the individual's income relative to the mean income, since $g^\star$ is implicitly given by the solution to (A.4):

$$b'(g^\star) = \frac{y_i}{\mu} \tag{A.5}$$

Because, by assumption, the marginal utility from g is decreasing in the level of g, the demand for (i.e. the optimal level of) the public good g is decreasing in individual income y_i.[35]

[35]Majority voting over any pair of alternative levels of g (and thus of levels of τ) necessarily leads to the solution that the median voter will be decisive, because each individual chooses the alternative closer to his optimal level of g and thus the median voter will choose whatever alternative is 'closer' to this individual optimal level of redistribution.

3.B Definitions of Variables

This section gives the definitions of the variables not already explained in the main text, unless their measurement is obvious anyway (e.g. age).

Relative income: Relative income is defined as $(y_i/\overline{y})$, where y_i is the personal net monthly income of individual i and $\overline{y}$ is the average net monthly income in the (analysis) sample.

Justice evaluation, coworkers' wage: This variable is measured as the ratio between just and actual wage for i's coworkers, that is $(y^{\star}(i)_{\text{Coworker}}/y(i)_{\text{Coworker}})$, where y_i denotes the actual net monthly income of one's own occupation, estimated by individual i and y_i' denotes the 'just' net monthly income for one's occupation. This variable closely corresponds the justice measures proposed by Jasso (1999).

Mobility: The only information about individual mobility is contained in two questions about the self–perception of the position today and the position ten years ago. Both are measured on a scale from 1 (bottom) to 10 (top). Mobility is simply defined as the difference between the two scores (position today minus position ten years ago).

Needs: This variable is meant to capture the extent to which an person thinks that one's needs should be important in determining their income (need principle). This variable is constructed as an univariate scale from the following two questions about which factors should be important in determining one's pay: (i) having a family and (ii) having kids.

Effort: This variable is meant to capture the equity principle. This variable is constructed as an univariate scale from five questions about which factors should be important in determining pay: (i) the effort and time needed to aquire education, (ii) having to supervise others, (iii) how 'good' one does his job, and (iv) how much effort one exerts in his job.

Ascribed skills: This variable measures the extent to which a person beliefs in ascribed factors as being important in determining the amount of compensation. This question relates to the perception of individuals of which factors actually are important for getting ahead: (i) have a wealthy family, (ii) know the 'right' people.

Acquired skills: This variable is the sum of over the two following questions: (i) people are paid according to their skills (ii) people are paid according to their effort.

SIOPS: This variable measures occupational prestige according to the Standard International Occupational Prestige Scale (SIOPS), as suggested by Treiman (1977). Also see Ganzeboom and Treiman (1996).

Conflict: This variable measures the perception of conflicts. Included items are questions about the existence of conflicts between: (i) rich and poor people, (ii) blue– and white–collar workers, (iii) managers and subordinates, (iv) young and old people, (v) people at the top and at the bottom.

Political scale: This variable measures the self–rated position on a scale between 0 and 10, where 0 (10) indicates the leftmost (rightmost) position.

3.C Derivation of the Gini Coefficient

For grouped wage data, the Gini coefficient gc can be computed as:[36]

$$\begin{aligned} gc &= \left(\left[\sum_{j=1}^{k} 0.5 \cdot (F_{j-1} + F_j) q_j\right] - 0.5\right) / 0.5 \\ &= \left[\sum_{j=1}^{k} (F_{j-1} + F_j) q_j\right] - 1 \end{aligned} \tag{C.1}$$

Where the k groups are ordered by their average within–group wage. F_j denotes to the accumulated population share up to (and including) group j and q_j represents the wage share of group j. If there are only two groups (i.e. $k = 2$), the computation simplifies considerably. Denoting $j = 1 =$ bottom and $j = 2 =$ top we immediately get:

$$\begin{aligned} gc &= [(0 + F_{\text{bottom}})q_{\text{bottom}} + (F_{\text{bottom}} + F_{\text{top}})q_{\text{top}}] - 1 \\ &= [(0 + f_{\text{bottom}})q_{\text{bottom}} + (f_{\text{bottom}} + 1)q_{\text{top}}] - 1 \\ &= f_{\text{bottom}}q_{\text{bottom}} + f_{\text{bottom}}q_{\text{top}} + q_{\text{top}} - 1 \\ &= f_{\text{bottom}}(q_{\text{bottom}} + q_{\text{top}}) + q_{\text{top}} - 1 \\ &= f_{\text{bottom}} - q_{\text{bottom}} \end{aligned} \tag{C.2}$$

The first equality of equation (C.2) follows from the fact that $F_0 = 0$, $F_1 = F_{\text{bottom}} = f_{\text{bottom}}$, and $F_2 = F_{\text{top}} = f_{\text{bottom}} + f_{\text{top}} = 1$. The last equality follows from the fact that $(q_{\text{bottom}} + q_{\text{top}}) = 1$ and thus $(q_{\text{top}} - 1) = -q_{\text{bottom}}$.

[36] This formula reflects the geometric interpretation of the Gini coefficient, being the ratio of the area between the curve representing equal distribution of wages and the Lorenz curve to the area under the curve representing equal distribution (which is equal to 0.5 by construction).

3.D Sensitivity Analysis

3.D.1 Sensitivity With Respect to Relative Population Shares

One obvious concern of the measures proposed in section 3.4 is that they all rely on the the assumption about the proportion of 'poor' individuals in the population. The subjective gini coefficient can be written as (supressing the indexing by i and denoting $f_{\text{bottom}} = f$ and $q_{\text{bottom}} = q$ for the sake of simplicity):

$$gc = f - q = f - \left[\frac{f\overline{y}_{\text{bottom}}}{f\overline{y}_{\text{bottom}} + (1-f)\overline{y}_{\text{top}}}\right] = f - \left[\frac{f\overline{y}_{\text{bottom}}}{f(\overline{y}_{\text{bottom}} - \overline{y}_{\text{top}}) + \overline{y}_{\text{top}}}\right] \quad \text{(D.1)}$$

For given wages $\overline{y}_{\text{bottom}}$ and $\overline{y}_{\text{top}}$, the Gini coefficient gc is a nonlinear function of f. Clearly, $gc = 0$ for either $f = 0$ or $f = 1$, and $gc > 0$ if $0 < f < 1$ (given that $\overline{y}_{\text{bottom}} < \overline{y}_{\text{top}}$). Partial differentiation of (D.1) yields:

$$\begin{aligned}\frac{\partial gc}{\partial f} &= 1 - \left[\left(\overline{y}_{\text{bottom}}\frac{1}{f(\overline{y}_{\text{bottom}} - \overline{y}_{\text{top}}) + \overline{y}_{\text{top}}}\right) - \left(\overline{y}_{\text{bottom}} f\frac{(\overline{y}_{\text{bottom}} - \overline{y}_{\text{top}})}{(f(\overline{y}_{\text{bottom}} - \overline{y}_{\text{top}}) + \overline{y}_{\text{top}})^2}\right)\right] \\ &= 1 - \left[\frac{\overline{y}_{\text{bottom}} \cdot \overline{y}_{\text{top}}}{(f(\overline{y}_{\text{bottom}} - \overline{y}_{\text{top}}) + \overline{y}_{\text{top}})^2}\right] \qquad \text{(D.2)}\end{aligned}$$

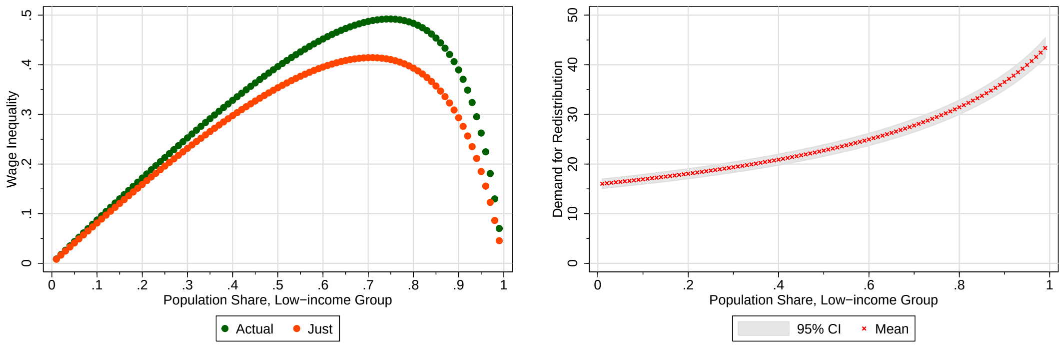
Which itself is also a nonlinear function of f. The second term on the right–hand side is equal to the partial derivative $\frac{\partial q}{\partial f}$, which has limits: $\lim_{f\to 0} \frac{\partial q}{\partial f} = \frac{\overline{y}_{\text{low}}}{\overline{y}_{\text{high}}}$ and $\lim_{f\to 1} \frac{\partial q}{\partial f} = \frac{\overline{y}_{\text{high}}}{\overline{y}_{\text{low}}}$, both of which are strictly positive for $\overline{y}_{\text{low}}, \overline{y}_{\text{high}} > 0$. Since it must be the case that $\overline{y}_{\text{low}} < \overline{y}_{\text{high}}$, $\frac{\partial q}{\partial f}$ monotonically increases with f. Further, because $\frac{\partial q}{\partial f}$ necessarily crosses the value 1 once, $\frac{\partial gc}{\partial f}$ first increases and then decreases in f and has a single maximum.[37] Figure D.1 shows the mean values of the two gini coefficients over the whole possible range of f ('simulated' by brute force for the whole sample). Figure D.1 also shows that actual inequality is (on average) always greater than the desired inequality, although $\text{gc}_{\text{actual}}$ could principally be higher or lower than gc_{just}, depending on the values of $\overline{y}_{\text{low}}$ and $\overline{y}_{\text{high}}$. This implies that the average demand for redistribution is always positive, regardless of the chosen value for f. Figure D.1 further shows that the average demand for redistribution is monotonically increasing in the population share of the bottom group f.[38]

Table D.1 shows estimation results for different (reasonable) values of f_{bottom} for the

[37] $\partial gc/\partial f = 0$ if $\partial q/\partial f = 1$, which implies that $\partial gc/\partial f = 0$ if $f = \left(\frac{\sqrt{\overline{y}_{\text{bottom}} \cdot \overline{y}_{\text{top}}} - \overline{y}_{\text{top}}}{(\overline{y}_{\text{bottom}} - \overline{y}_{\text{top}})}\right)$.

[38] This is the case because, conditional on f, gc further depends only on the ratio of the two group wages (that is, on the wage shares of the two groups). Empirically, as discussed in main text, the desired wage share is higher than the perceived wage share of the bottom group, such that for each $f \in (0, 1)$ the gc for the actual distribution is higher than the gc describing the just distribution.

Figure D.1: Wage inequality for different values of f_{bottom}



Notes: The figure on the left shows sample averages for the two subjective inequality measures over different values for f_{bottom}. The figure on the right shows the average demand for redistribution for different values of f_{bottom}.

main empirical model (see table 3.7 in the main text). The two rows at the top show that not only the mean, but also the variance of the redistribution measure monotonically increases with the population share of the bottom group f. Most importantly though, table D.1 shows that the qualitative pattern of the estimates does not depend on the value of f.

3.D.2 Sensitivity with Respect to Sample Selection

Another point in question is whether the inequality measures or the regression results depend on the the sample of observations chosen. Table D.2 reports parameter estimates for the main model, estimated over different samples of observations. The first column of table D.2 replicates the main result from the main estimation model (see table 3.7). Columns two and three show estimates for the same model, but each with an additional sample restriction. Column 2 adds the restriction that only individuals are included which gave estimates for actual and just wages for the same group of occupations. Column 3 restricts the sample to individuals who gave estimates for all (nine) occupations. Since there are considerable within–subject differences concerning the extent of occupational compensation, the subjective inequality measures might depend on the pattern of occupational wage estimates (that is, measures describing the actual and the desired wage distribution might differ, for the same individual, because the pattern of available wage estimates differs).

In the remaining three columns of table D.2, an attempt is made to increase the number of observations by a very simple method of missing data imputation. For each regressor, missing values were replaced by the mean value of the corresponding variable, and an indicator variable is created for each regressor, taking on the value 1 if the observation has missing information about that variable (and 0 otherwise).[39] Comparing columns one and four, we see that this procedure increases the sample size by more than 50%, without changing the qualitative pattern of estimates. The same is true if we only consider observations with specific patterns of occupational wage estimates.

Below the line then, although a lot of observations is lost due to missing values on all or part of the regressors, the qualitative pattern of the main regression model is not changed.

[39] This way of dealing with missing data on covariates essentially maximizes the sample size without imputing missing values.

Table D.1: Sensitivity analysis with respect to f_{bottom}

	Demand for redistribution						
Mean	24.954	26.299	27.825	29.574	31.608	34.011	36.918
Standard deviation	18.029	18.503	19.018	19.580	20.199	20.890	21.681
Relative income	−5.610***	−5.801***	−6.005***	−6.224***	−6.455***	−6.695***	−6.935***
	(1.509)	(1.562)	(1.619)	(1.683)	(1.754)	(1.836)	(1.930)
Justice evaluation	5.180***	5.273***	5.357***	5.427***	5.474***	5.483**	5.433**
	(1.965)	(1.997)	(2.032)	(2.070)	(2.113)	(2.162)	(2.222)
Mobility	−0.714	−0.742	−0.773	−0.806	−0.841	−0.880	−0.920
	(0.493)	(0.506)	(0.519)	(0.534)	(0.551)	(0.570)	(0.593)
Needs	1.676**	1.682*	1.683*	1.677*	1.659*	1.624	1.564
	(0.847)	(0.869)	(0.894)	(0.922)	(0.954)	(0.993)	(1.041)
Effort	−4.225***	−4.370***	−4.523***	−4.684***	−4.852***	−5.025***	−5.199***
	(1.634)	(1.676)	(1.722)	(1.773)	(1.830)	(1.896)	(1.974)
Ascribed skills	1.769*	1.834*	1.908*	1.991*	2.087*	2.199*	2.331*
	(1.020)	(1.042)	(1.065)	(1.091)	(1.121)	(1.155)	(1.195)
Aquired skills	−1.742	−1.872	−2.022*	−2.197*	−2.404*	−2.654*	−2.964**
	(1.128)	(1.165)	(1.206)	(1.252)	(1.302)	(1.360)	(1.429)
Additional controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
f_{low}	0.65	0.70	0.75	0.80	0.85	0.90	0.95
n	581	581	581	581	581	581	581
Adjusted R^2	0.168	0.169	0.169	0.169	0.169	0.167	0.163
p–value (F–statistic)	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Notes: *, **, *** denotes statistical significance on the 10%, 5%, and 1% level, respectively. Robust standard errors in parentheses. Also see the notes of table 3.7. Own calculations, based on ISSP (1999).

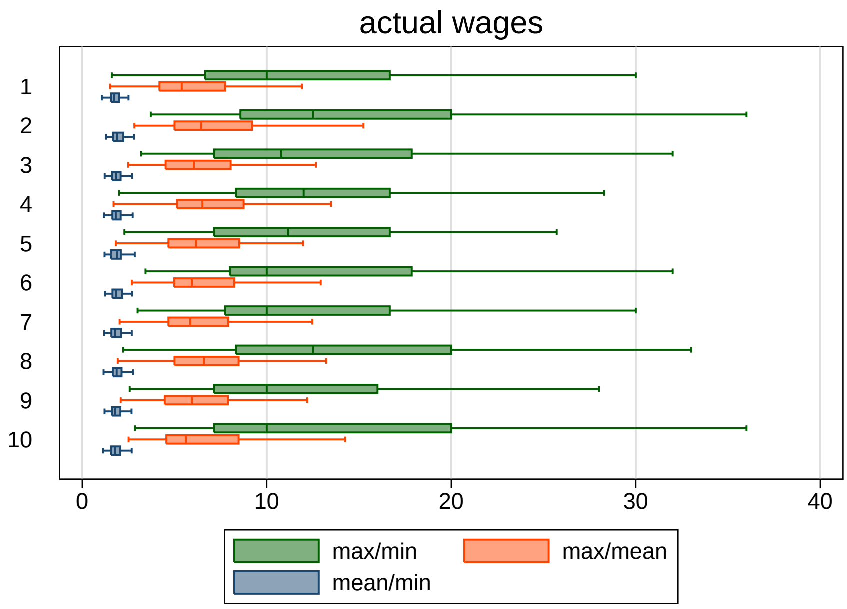
Table D.2: Sensitivity analysis

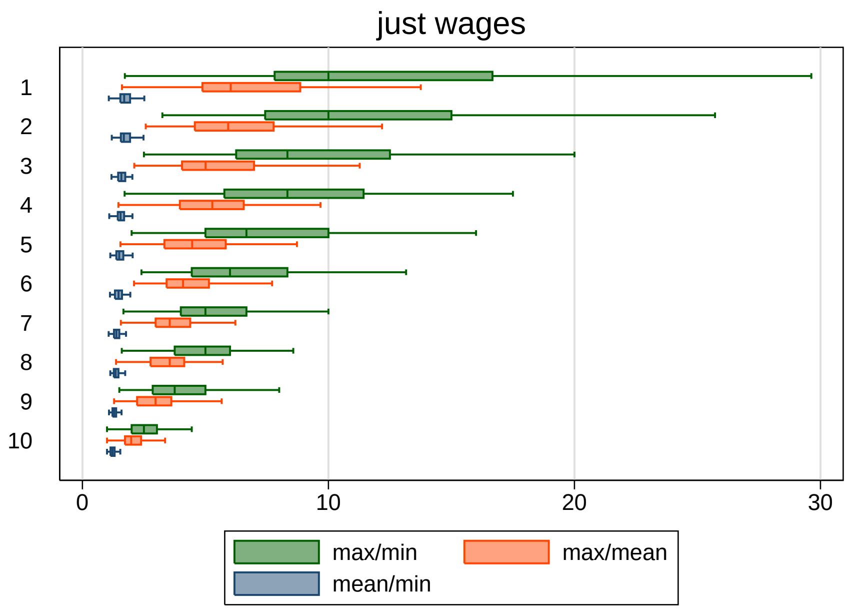
	Demand for redistribution					
Mean	36.918	37.400	37.367	36.537	37.284	37.524
Standard deviation	21.681	21.139	21.203	25.046	22.063	21.791
Relative income	−6.935***	−8.668***	−8.268***	−6.941***	−7.144***	−6.594***
	(1.930)	(1.706)	(1.732)	(1.759)	(1.616)	(1.636)
Justice evaluation	5.433**	5.304***	5.451**	3.438*	5.176***	5.216**
	(2.222)	(1.966)	(2.118)	(1.909)	(1.952)	(2.109)
Mobility	−0.920	−0.944	−1.130*	−1.114**	−0.598	−0.676
	(0.593)	(0.618)	(0.645)	(0.555)	(0.484)	(0.503)
Needs	1.564	1.458	1.332	2.575***	2.650***	2.503***
	(1.041)	(1.118)	(1.191)	(0.817)	(0.873)	(0.922)
Effort	−5.199***	−2.740	−1.391	−7.792***	−5.910***	−4.747***
	(1.974)	(2.022)	(2.133)	(1.882)	(1.704)	(1.826)
Ascribed skills	2.331*	2.225*	2.194*	−0.069	0.974	1.312
	(1.195)	(1.205)	(1.243)	(1.137)	(1.056)	(1.096)
Aquired skills	−2.964**	−2.267	−1.517	−2.680**	−2.147*	−2.136*
	(1.429)	(1.456)	(1.507)	(1.177)	(1.215)	(1.250)
Occupational wage data						
Equal pattern only?	No	Yes	No	No	Yes	No
Full pattern only?	No	No	Yes	No	No	Yes
Observations with missing covariates						
Included?	No	No	No	Yes	Yes	Yes
Additional controls?	Yes	Yes	Yes	Yes	Yes	Yes
n	581	516	478	935	799	712
R^2	0.192	0.212	0.216	0.174	0.178	0.187
p–value (F–statistic)	0.000	0.000	0.000	0.000	0.000	0.000

Notes: *, **, *** denotes statistical significance on the 10%, 5%, and 1% level, respectively. Robust standard errors in parentheses. Also see notes of table 3.7. Own calculations, based on ISSP (1999).

3.E Additional Figures and Tables

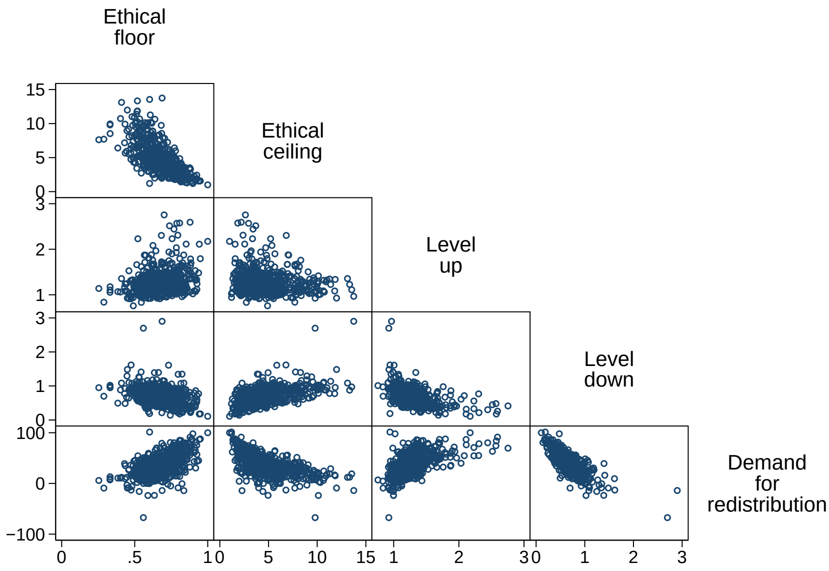
Figure E.1: Moments of subjective wage distributions, over deciles of the demand for redistribution





Notes: The figures show the distribution of actual (top) and desired (bottom) wages within deciles of the demand for redistribution (given by the y–axis). Own calculations, based on ISSP (1999).

Figure E.2: Scatterplots of various moments describing the perceived (desired) wage distribution



Notes: All variables are defined in the main text. Own calculations, based on ISSP (1999).

Table E.1: Summary statistics

	Analysis sample	Whole sample
A. Financial self–interest & mobility		
Net monthly income (SFr.)	5029.98	4467.16
	(2904.78)	(2717.41)
Fairness evaluation (coworkers' wage)	0.13	0.14
	(0.30)	(0.35)
Mobility index	0.57	0.42
	(1.61)	(1.63)
B. Social norms & perceptions		
Needs	3.24	3.34
	(1.01)	(0.99)
Effort	3.81	3.83
	(0.45)	(0.46)
Ascribed skills	3.02	2.98
	(0.76)	(0.75)
Acquired skills	3.32	3.31
	(0.61)	(0.64)
C. Additional control variables		
SIOPS	46.65	44.52
	(12.35)	(12.04)
Employed full–time (dummy)	0.58	0.44
	(0.49)	(0.50)
Employed (dummy)	0.85	0.67
	(0.36)	(0.47)
Not employed (dummy)	0.13	0.30
	(0.34)	(0.46)
Age (years)	43.27	45.15
	(13.44)	(14.82)
Female (dummy)	0.41	0.54
	(0.49)	(0.50)
Education (years)	13.45	12.86
	(2.64)	(2.61)
Urban residence (dummy)	0.65	0.66
	(0.48)	(0.47)
Living in German–speaking part (dummy)	0.73	0.71
	(0.44)	(0.45)
Foreign citizenship (dummy)	0.14	0.16
	(0.35)	(0.36)
Political self–assessment	4.78	4.86
	(1.68)	(1.66)
Perception of conflicts	2.33	2.38
	(0.48)	(0.52)

Notes: The first column shows mean values of the control variables for the subsample which is used in all analyses, the second column shows mean values of the control variables for the whole sample. Standard deviations in parentheses.

Table E.2: Full regression results

	Demand for redistribution	
Relative income	−6.731***	−7.035***
	(1.453)	(1.920)
Justice evaluation (coworker)	5.086**	5.479**
	(2.166)	(2.228)
Mobility	−0.289	−0.904
	(0.583)	(0.592)
Needs	2.190**	1.568
	(0.983)	(1.041)
Effort	−8.260***	−5.348***
	(1.809)	(1.966)
Ascribed skills	2.053*	2.288*
	(1.230)	(1.193)
Aquired skills	−3.696***	−2.794**
	(1.387)	(1.412)
SIOPS		0.044
		(0.074)
Employed full–time (yes = 1)		0.815
		(2.375)
Employed (yes = 1)		−4.063
		(4.339)
Not employed (yes = 1)		−4.294
		(4.701)
Age (years)		0.616
		(0.436)
Age squared		−0.009*
		(0.005)
Female (yes = 1)		0.425
		(2.023)
Education (years)		0.764**
		(0.382)
Living in urban area (yes = 1)		−1.951
		(1.840)
Living in German–speaking are (yes = 1)		−0.766
		(1.892)
Foreign citizenship		−5.973***
		(2.181)
Political self–assessment		−1.452***
		(0.550)
Perception of conflicts		5.631***
		(1.925)
Constant	73.657***	40.468**
	(9.007)	(16.293)
n	581	581
Adjusted R^2	0.110	0.164
p–value (F–statistic)	0.000	0.000

Notes: *, **, *** denotes statistical significance on the 10%, 5%, and 1% level respectively. Robust standard errors in parentheses.

CHAPTER

FOUR

COMPENSATING WAGE DIFFERENTIALS & THE VALUE OF A STATISTICAL INJURY

Joint with Oliver Ruf

"A life which included no improbable events would be the real statistical improbability."

Poul Anderson (1926–2001), American writer

4.1 Introduction

It has long been recognized in economics that differences in wages are not only due to the fact that individuals differ in their productivity–relevant characteristics (e.g. education), but also due to the fact that the jobs offered to workers differ enormously along various dimensions (workplace safety being only one important example). Workers presumably not only value the monetary payoff from working, but also the non–monetary characteristics, potentially giving rise to compensating wage differentials (Rosen, 1986). This means firms offering jobs with "negative" characteristics, that is, attributes to which workers attach a negative value, must attract workers by paying them higher wages ceteris paribus, thus "compensating" them for the negative aspects of the job (and vice versa for "positive" workplace characteristics). Non–monetary characteristics of jobs are of course manifold, most empirical studies though focus on workplace safety, that is on the risk compensation for both fatal and non–fatal accidents (e.g. Viscusi and Aldy, 2003).

The theory of compensating wage differentials has inspired a huge number of empirical studies trying to pin down the compensation for undesirable workplace attributes. Due to the implicit trade–off between job amenities and wages, observed (or rather, estimated) compensating wage differentials can be used to assess the value of a statistical life or injury, respectively. These empirical results in turn may directly influence public policy, since cost–benefit analyses with respect to safety regulations need empirical assessments on the monetary value of such regulations (this applies not only to regulations of safety at the workplace, but also to environmental regulations for example).

Yet, the intuitive appeal of the theory notwithstanding, empirical studies face some fundamental problems concerning the identification of compensating wage differentials. The main problem is rooted in unobserved productivity differences between individuals and the thereupon based sorting of workers into jobs with different risks (due to the positive income elasticity of the value of a statistical accident). This presumably explains the rather large variation in the estimated compensation for risks on the one hand, but also the fact that many empirical studies report no compensation for risk or even report compensating wage differentials having the "wrong" sign (at least with respect to non–fatal injury risks). For example, the survey by Viscusi and Aldy (2003) reports a rather wide range of estimates for the value of a statistical injury from about \$20,000 to \$70,000 (for the United States only).

This chapter presents empirical evidence on the compensation for non–fatal accident risk in Switzerland, using a data set compiled from two different sources (which we will discuss in detail below). Our study has three main features. *First*, we will exclusively focus on non–fatal accidents. This focus reflects the fact that most accidents have (fortunately) non–fatal consequences and thus, from the viewpoint of public health and safety, merit the most attention.[1] In the year 2004 (the year of our empirical analysis), for example, the Swiss Accident Insurance Fund reports about 246,000 non–fatal accidents related to work but only 188 fatal accidents. *Second*, we observe the number of non–fatal accidents not only within entire industries, but also within cells defined by industry$\times$skill–level of the job. This is a tremendous advantage from an empirical point of view, since risks at (too) high levels of aggregation mix the risks of very different groups of workers and different willingness to pay for avoiding risk, which might lead to biased estimation of the compensation for risk in the workplace. *Third*, we capitalize on the availability of longitudinal wage information, which allows us to use simple panel estimation methods in order to isolate the firm wage component. We believe that our empirical approach, on the one hand using the number of non–fatal

[1] Our focus though is also due to the available data on non–fatal accidents as well as the empirical approach we take, as we will discuss in detail in section 4.3 and section 4.4 below.

accidents within narrower cells than usually available, and on the other hand combining panel data estimation methods with simple non–parametric stratification, transcends the typical hedonic wage function approach often used in the literature on the subject.

The main findings of our empirical analysis are the following. *First*, we find that a simple hedonic wage regression, where the observed log wage is regressed on the risk measure (and additional control variables) yields a compensation for non–fatal accident risk which is statistically zero, a result that is in line with some previous empirical studies. The leading explanation for this result (which runs counter to theory) is presumably the sorting of workers which differ in their unobserved productivity. *Second*, moving on to, in a sense, more sophisticated (but, we believe, in this case also more reliable) methods, we find a positive point estimate for the compensation of non–fatal accident risk. Our preferred point estimate yields an implicit value of a statistical injury of about 40,000 Swiss francs (which lies well within the range given by studies from the U.S. labor market, as well as from studies outside the U.S.). On the other hand, using different estimation methods yields considerably different values for the value of a statistical injury. As we will discuss later, a significant cause of this wide range of estimates is the difference in the estimation methods used. *Third*, comparing the different estimation methods may shed some light on the problem of endogenous sorting of workers into jobs with different risks, which presumably yields biased estimates for the compensation of risk. Our results are in fact in line with the argument supported by Hwang *et al.* (1992), among others, that such endogenous sorting gives rise to severe *underestimation* of risk compensation. *Fourth*, we find significant differences between men and women on the one hand, and between smaller and larger firms on the other hand with respect to the compensation of non–fatal accident risk. *Fifth* and finally, our results also show that the kind of risk–data available can make an important difference for the empirical assessment of risk compensation.

The rest of this chapter is organized as follows. We start with a discussion of the relevant literature on compensating wage differentials, focusing on empirical studies estimating the compensation for non–fatal accident risk. In section 4.3, we discuss the two data sources we rely on, discuss the construction of the variables of main interest – along with some descriptive statistics. We then expore issues of identification and estimation in section 4.4. Specifically, we will discuss three different approaches to identification and estimation. We start with a simple hedonic wage regression model, where the wage is simply regressed on individual– and firm–specific characteristics. The second approach is based on the idea that we can control for unobserved heterogeneity of individuals by appropriately stratifying the sample. The third approach we take capitalizes on the longitudinal structure of the wage data. We isolate the wage component, which is specific to the firm and then use only this part of the wage to estimate risk compensation. The results of the different estimation methods are presented and discussed in section 4.4. Based on our econometric results, we further present estimates of the value of a statistical (non–fatal) accident in Switzerland. Section 4.6 concludes.

4.2 Related Literature

4.2.1 Compensation for Workplace Accident Risk

There is a large number of empirical studies which try to pin down the compensation for accident risks, as well as for a wide range of other job amenities and disamenities (the surveys by Viscusi and Aldy (2003) and Viscusi (1993) are of special interest here; see also the more recent, but less thorough survey by Ashenfelter (2006)).[2] Most empirical studies find a positive compensation for fatal accident risk, often yielding high implicit values of a statistical life. For example, Viscusi and Aldy (2003) report that half of the studies from the U.S. labor market surveyed in their article give a value of a statistical life within the range of \$3.8–\$9.0 million (in 2000 dollars), the median estimate being about \$7 million. Most studies from outside the U.S. labor market give estimates within the same range. It is difficult to assess the exact reasons for this wide range of estimates, since the studies differ in various ways, for example with respect to the available data and risk measure[3], or in the econometric methods applied.

The evidence on the compensation for non–fatal accident risk is much less coherent, which is somewhat surprising since most studies that present estimates of such compensation are based on the same data as estimates for the compensation for fatal accident risk. Viscusi and Aldy (2003) report, for both the U.S. as well as other labor markets, a probable range for the value of a statistical injury of about \$20,000–\$70,000 per injury.

4.2.2 Endogenous Sorting

The main problem from the empirical point of view is the potential sorting of workers into jobs differing in their risk of accidents. Hwang *et al.* (1992), among others, argue that the problem of main concern are differences in unobservables which in turn relate to the

[2] Compensating wage differentials have also been found, for example, for the risk of unemployment (Lalive *et al.*, 2006; Moretti, 2000), for shift work (Kostiuk, 1990), and uncertainty with respect to future earnings (Feinberg, 1981).

[3] Most importantly perhaps, some studies rely not on direct measures of risk (i.e. number of accidents), but base their analyses on tradeoffs outside the labor market, e.g. on the tradeoff between traffic accidents and the price of automobiles (Dreyfus and Viscusi, 1995) or fatalities related to bicycle accidents and the prize of bicycle helmets (Jenkins *et al.*, 2001). Other studies have used subjective assessments of risk, as for example Viscusi and O'Connor (1984) and Viscusi and Hersch (2001).

productivity of workers and thus may lead to sorting of workers into jobs with different risks. The sorting of workers in turn is endogenous due to the fact that the income elasticity of the value of a statistical life or injury is positive, i.e. more productive workers sort themselves into less risky jobs by accepting ceteris paribus lower wages. Viscusi and Aldy (2003), for example, report an income elasticity of about 0.5–0.6. On the other hand though, Shogren and Stamland (2002) argue that the bias in estimating the compensating wage differential could run in the other direction, assuming that workers not only differ in their productivity, but also with respect to their skill in avoiding accidents. Thus, workers in risky jobs could be either more tolerant to risk or more skilled in avoiding risk (or both). Thus they show that the estimated risk compensation might actually be upward biased, rather than downward biased.

Some studies have tried to approach the problem of endogenous sorting by using instrumental variables (DeLeire and Levy, 2004; Garen, 1988, for example,). The study by Garen (1988), for example, tries to correct for the endogeneity of job risk by using a system of simultaneous equations where marital status and the number of dependents are used as instruments for the preference over risk.

4.2.3 Measurement of Risk

Another empirical issue concerns the measurement of the risk of an accident. First, as pointed out by Mellow and Sider (1983) for example, typical survey data are more often than not plagued by measurement error, i.e. it seems to be the case that workers often misreport their industry affiliation and/or their exact occupation. Assuming that this kind of measurement error is random, this causes the compensating differential to be biased towards zero. Second, there clearly is a trade–off of the following form. On the one hand, risk measurements at a low level of aggregation are preferred, as otherwise one might mix workers with very different occupations into the same risk categories. On the other hand though, risk measures at a low aggregation level run into the problem that many cells will have zero risk, at least for shorter periods of time. This is specifically true for fatal accident risk, yet obviously also applies to non–fatal injuries.

4.2.4 Estimation

The most prevalent approach in the empirical literature is via estimation of hedonic wage functions, that is, by running regressions of the wage on characteristics of both the workers *and* jobs. As we will make explicit in section 4.4, this approach is likely to fail identification because it is unlikely that this approach can effectively deal with the problem of endogenous sorting of workers into jobs (as pointed out above, some studies have tried to instrument endogenous sorting by using family characteristics).

As we will discuss in detail in section 4.4 below, our empirical approach of choice relies on the panel structure of the wage data. Thus, our study also relates to work on matched employer–employee data (e.g. Abowd and Kramarz, 1999) as well as the panel data estimation methods in general (e.g. Wooldridge, 2002). We cannot directly apply the methods of Abowd and Kramarz (1999) though, because our wage data has a longitudinal structure only with respect to the employer, but not with respect to the individual worker.

4.2.5 Empirical Evidence for Switzerland

To the best of our knowledge, there is only a single published study on the compensation of accident risk for Switzerland by Baranzini and Ferro-Luzzi (2001), focusing on fatal accident risk only. They report estimates for the value of a statistical life ranging from about 12 to about 32 million Swiss francs. Besides having a different focus (our focus in on non–fatal accident risk only), our study differs in at least two further ways. *First*, we have access to the number of non–fatal accidents not only within industries, but within narrower cells, defined over industry×skill–level of the job. *Second*, we do not primarily and exclusively rely on simple hedonic wage regressions for the estimation of risk compensation, instead we use the longitudinal structure of the wage information in a first stage in order to deal with the endogenous sorting of workers.

4.3 Data

4.3.1 Data Sources

We use two different data sources. The first data source is the Swiss Wage Structure Survey (SWSS; "Lohnstrukturerhebung (LSE)"), which is a biannual survey among firms which is administered and made available by the Swiss Federal Statistical Office. The SWSS is one of the two largest official surveys in Switzerland focused mainly on employment–relevant information.[4] The SWSS is a survey of firms, covering the population of large firms along with a random sample of small firms. We use three different waves of the SWSS (from the years 2000, 2002, and 2004) and we extract individual

[4] The second important labor market survey is the Swiss Labor Force Survey (SLFS; "Schweizerische Arbeitskräfteerhebung (SAKE)"). The two main advantages of using the SWSS over the SLFS are the following: First, the SWSS allows isolating the wage firm fixed effect, which is the part of the observed wage where risk compensation should show up. Second, the SWSS is (opposed to the SLFS) mailed to employers, and thus misclassification of occupations and industries should only be of minimal order (the same is arguably true for wages).

monthly earnings along with several individual–specific characteristics (see section 4.3.4 below) on details.

Our risk measure corresponds to the number of non–fatal accidents within cells defined over industry (forty different industries on a two–digit level) and skill–level of the job (four different levels). The data have been provided by the Swiss Accident Insurance Fund (SAIF; "Schweizerische Unfallversicherungsanstalt (Suva)"), which is the most important accident insurance fund in Switzerland. The number of non–fatal accidents within industry×skill–level cells are available for the year 2004.

4.3.2 Definitions

One of the main features of our analysis is that our risk measure r_k gives the number of non–fatal accidents per year and per 1,000 workers within a given industry×skill–level cell k (instead of within–industry only). Data on the absolute number of non–fatal accidents for the year 2004 is available within cells defined over industry×skill–level of job. Now, because the SAIF does not directly have the number of workers within these cells and because workers are not uniformly distributed over these cells, we also need to know the distribution of workers over these cells in order to compute the risk of a non–fatal accident. To this end, we simply use the distribution of workers in the SWSS (from the year 2004), and then approximate the population distribution of workers by multiplying the number of workers within a given cell with the total number of workers which are covered by the SAIF (about 1.827 millions in the year 2004).

Note that there is a fundamental trade–off with respect to the risk measure chosen: On the one hand, risk measures on a highly disaggregated level are preferred, such that we do not pool accident risks of individuals working in very different occupations and jobs. This has been pointed at, for example, by Viscusi (1993, p.1928), noting that "[t]he main deficiency of industry–based data is that they pertain to industry–wide averages and do not distinguish among the different jobs within that industry [...]". On the other hand, accidents observed at a very low level of aggregation also give rise to estimation problems, because the number of accidents tends towards zero for most cells if we shrink the size of the risk–relevant cells. That, in fact, is the reason why we decided not to use the information about fatal accidents for this study. Disaggregating the number of fatal accidents over the skill–level of job actually yields far too many cells with zero number of accidents.

The SWSS includes average gross monthly wages for full–time employment (i.e. 172 hours per month), including mandatory social security contributions and extra pay (e.g. for night work, 13. monthly wage). The SWSS also includes several socio–demographic characteristics (e.g. age, gender, tenure, educational attainment (highest degree), citizenship), but also different firm characteristics (most importantly, the size of the firm along with the geographic location).

4.3.3 Measurement Error

One main advantage of our data is that measurement error in the risk data and industry–affiliation of workers is arguably of minor significance (as already mentioned in section 4.2, Mellow and Sider (1983) have pointed out the problem of misclassification of both industry and occupation). This is important because measurement error in the risk variable tends to bias the compensating wage differential towards zero (measurement error in the dependent variable (i.e. wage) is, of course, also common but of less concern). We are confident that the measurement error for both our risk measure and industry–affiliation is of no great importance, since the SWSS does not involve employees but obtains the data from the employer directly (such that misclassification of either industry and/or occupation is unlikely to occur). For the same reason, we also believe that our wage information is more reliable than the information available in typical survey data (although presumably less reliable than administrative data). Additionally, our risk measure is directly obtained from administrative sources and should thus cover all relevant accidents.

4.3.4 Descriptive Statistics

Table 4.1 shows descriptive statistics for both the overall sample as well as the sample of individuals in jobs of the lowest skill–level (that will be used in the empirical analysis discussed below). In both samples, we only consider workers aged between 16 and 64 (for men) and between 16 and 61 (for women). A second restriction applies to the size of the employer. Because we are estimating wage fixed effects for each firm, we also restrict the sample to workers from firms which have at least ten workers in each of the four job skill–levels in each year. The overall sample includes more than one million individual workers, the subsample of workers in the lowest skill–level (with respect to the job, *not* with respect to the educational attainment of the worker) consists of about 300,000 individual workers. In both cases, there are about 3,500 different firms (due to the restriction on firms). As we will discuss in–depth in section 4.4 below, our preferred estimation approach will focus exclusively on workers within a given skill–level as collected in the SWSS, as we believe that such a stratification of the workers yields more reliable estimates of the compensating wage differential.

We begin with describing the overall sample, which is representative of the Swiss labor market as a whole. The typical worker in the Swiss labor market has gross earnings equal to 6,300 Swiss francs a month, is about 40 years old and has about 9.5 years of tenure and is more likely to be a man. The average employer has more than 2,800 workers (reflecting the sampling structure of the SWSS as well as the restriction with respect to the selection of the employers). About two thirds of the workers are married, the other third single. The distribution of workers with respect to educational attainment highlights two important characteristics of the Swiss labor market in terms of education. First, compared to other countries, the number of workers with tertiary education is rather low (e.g. only about 5.5% of the workers have a university degree). Second, about half of the workers hold a vocational training. Another important characteristic of the Swiss labor market is the large fraction (about 20%) of workers without Swiss citizenship. Focusing on individuals working in jobs with the lowest skill–level (columns 3 and 4 of table 4.1) yields the expected result that some groups are overrepresented in the analysis sample relative to the overall sample of individuals (although this subset of individuals is similar to the overall sample with respect to some characteristics, for example age and size or the geographic location of the employer).[5] Here, average monthly earnings are only about 70% of the overall average earnings (about 4,500 Swiss francs). Moreover, a worker from skill–level four is more likely to be a woman, more likely to be married and much more likely not to have Swiss citizenship, compared with a worker from the overall sample. The most striking difference between the overall sample and the lowest skill–level sample though is the distribution of workers with respect to educational attainment. As table 4.1 shows, there are practically no workers with an educational degree above vocational training. This, in fact, is a desired result with respect to the empirical approach we take (see section 4.4 below): Given that education (of course not exclusively) reflects differences in productivity, focusing on workers with similar educational attainment also implies that these workers are more similar with respect to unobserved productivity–relevant characteristics (compared to workers from all job skill–levels). We believe that the variance of unobserved productivity is presumably lowest within the group of workers in the lowest skill–level (although this presumption obviously is fundamentally empirically untestable).

As table 4.1 also shows, the typical worker in the year 2004 was faced with the risk of a non–fatal, work–related accident of about 8.8% (88 accidents on average per 1,000 workers). In the sample of workers with lowest skill–level, the average risk was about half (about 43 accidents per 1,000 workers). Figure 4.1 shows a simple scatterplot between the average logarithmic monthly wage and the number of non–fatal accidents for workers

[5] The distribution of workers over the skill–level of jobs looks as follows: About 6% work in the highest level, about 20% in the second–highest level. 46% work in skill–level 3, and the remaining 28% of the workers are in jobs of lowest skill–level.

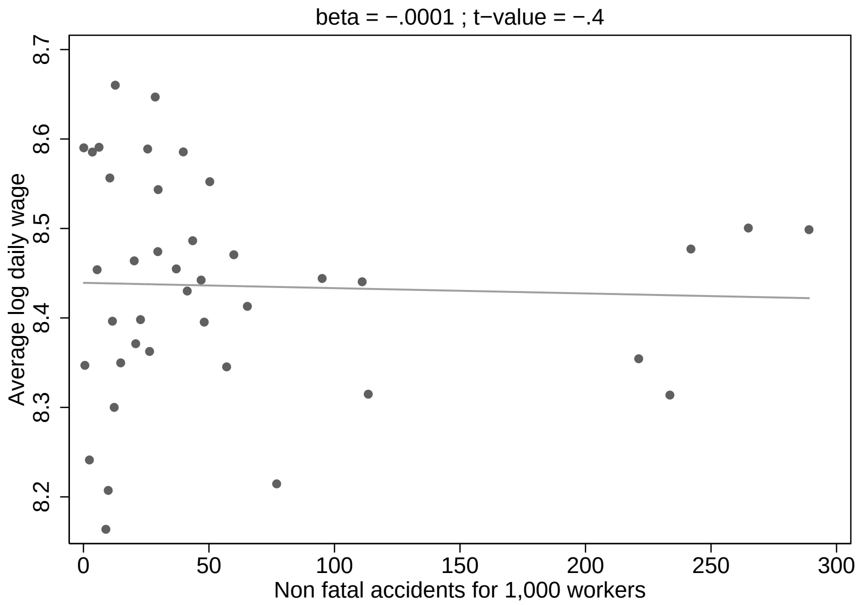
Table 4.1: Descriptive statistics

	Skill-level 1		Skill-level 1-4	
	Mean	Std. Dev.	Mean	Std. Dev.
Monthly wage	4526.63	1069.26	6371.88	3466.72
Natural logarithm of monthly wage	8.39	0.23	8.68	0.38
Non fatal accident risk (per 1,000 workers)	45.40	59.13	93.01	150.42
Age	40.19	11.66	40.71	11.14
Female	0.54	0.50	0.42	0.49
Tenure	7.63	8.18	9.06	9.12
Size of the firm	2714.94	7820.84	3108.01	7890.73
Marital status				
Single	0.27	0.44	0.32	0.47
Married	0.62	0.49	0.58	0.49
Others	0.11	0.31	0.10	0.30
Education				
University degree	0.00	0.05	0.06	0.23
College of higher education	0.00	0.05	0.05	0.21
Higher professional degree	0.01	0.08	0.07	0.26
Teachers' certificate	0.00	0.04	0.00	0.07
High School	0.01	0.11	0.02	0.14
Finished professional education	0.27	0.45	0.50	0.50
Firm intern professional education	0.14	0.34	0.07	0.25
Secondary school	0.48	0.50	0.18	0.38
Other degree	0.08	0.28	0.05	0.22
Citizenship				
Swiss citizenship	0.52	0.50	0.68	0.47
Short tem residence authorization	0.01	0.11	0.01	0.08
Long term residence authorization	0.08	0.28	0.05	0.23
Permanent recidence permit	0.29	0.45	0.17	0.37
Cross-border commuter	0.06	0.24	0.07	0.25
Others	0.03	0.18	0.03	0.16
Geographic region				
VD, VS, GE	0.19	0.39	0.16	0.37
BE, FR, SO, NE, JU	0.23	0.42	0.21	0.41
BS, BL, AG	0.12	0.33	0.14	0.35
ZH	0.24	0.42	0.27	0.44
GL, SH, AR, AI, SG, GR, TG	0.11	0.32	0.11	0.32
LU, UR, SZ, OW, NW, ZG	0.07	0.26	0.07	0.26
TI	0.04	0.19	0.03	0.18
Number of Firms	3,533		3,533	
Number of Observation	130,976		468,328	

Notes: Columns 1 and 2 refer to the subsample of workers in jobs of lowest skill–level, columns 3 and 4 to the full sample of workers. Sources: All variables are taken from the SWSS, except the number of non–fatal accidents. Risk measure gives the number of non–fatal accidents per 1,000 workers per year, within cells over industry×skill–level. Own calculations, based on SWSS (2004) and SAIF (2004).

from the lowest skill–level jobs at the level of industry×skill–level. The scatterplot shows no relation whatsoever between the two variables (if anything, the correlation goes the "wrong" way), which is underlined by the estimated slope coefficient from a regression of the average log earnings on the number of accidents – yielding essentially a zero point estimate, both in economic and statistical terms (t–value is approximately zero). This

Figure 4.1: Log–Wage versus non–fatal injury risk, by industry



Notes: The y–axis shows the average logarithm of monthly gross earnings and the number of non–fatal accidents per 1,000 workers per year. Workers in lowest job skill–level only. Table A.1 in the chapter appendix shows the corresponding numbers. Own calculations, based on SWSS (2004) and SAIF (2004).

result is not especially surprising though since average wages within industries clearly may not only reflect differences with respect to accident risks, but also differences in the composition of workers and jobs. We thus now move on to issues of identification and econometric estimation.

4.4 Identification and Estimation

We now discuss issues of identification and estimation of the compensating wage differential for (non–fatal) accident risk. We start with a simple hedonic wage regression of the following form:

$$y_{ijk} = \alpha + x_i'\beta + z_j'\gamma + \delta r_k + u_{ijk} \tag{4.1}$$

Where y_{ijk} is the natural logarithm of the gross monthly wage of individual i, working in firm j and industry×skill–level cell k. x is a (column) vector of individual characteristics including citizenship, educational attainment, age (and its square), tenure (and its square), a gender–dummy and marital status. z is a (column) vector of characteristics describing the firm (and thus reflecting the characteristics of the job), and includes the size of the firm (and its square) and the geographical location of the firm. r is our risk measure, corresponding to the number of non–fatal accidents in industry×skill–level cell k per 1,000 workers in the year 2004. u_{ijk} is the unobserved error term, upon which identification of the compensating wage differential obviously critically hinges.

α, β, γ and δ are parameters to be estimated from the sample data at hand. The constant term α is, of course, of no special interest but simply serves the purpose of scaling the expected value of the error term to zero. The two parameter vectors β and γ are also, for the purpose of our analysis, of no particular interest. The parameter of main interest is δ, which, under appropriate assumptions, corresponds to the compensating wage differential for non–fatal accident risk.

As explained in section 4.3, the number of non–fatal accidents is only available for a single point in time, so that we can essentially only run a cross–sectional hedonic wage regression[6] (but we do have a partial panel structure with respect to wages, which we will try to capitalize on later; see section 4.4.4 below).

4.4.1 Unobserved Heterogeneity and Worker Sorting

Parameter δ (as are the other parameters) is identified if we are willing to assume that:

$$\mathbb{E}(u|x, z, r) = \mathbb{E}(u) \tag{4.2}$$

This means, if we can safely assume that the error term u_{ijk} is mean independent of (x, z, r), then all the parameters of the regression given by equation (4.1) are identified. However, as has been pointed out by several authors (e.g. Hwang *et al.*, 1992) and discussed in section 4.2, there is good reason to act on the assumption that there is unobserved individual heterogeneity related to wages (that is, these differences somehow reflect differences in productivity not taken into account for by observed variables) *and* that "safety" is a normal good (i.e. the demand for "safety" increases as income rises). Thus, workers of high productivity sort themselves into less risky jobs by accepting lower

[6]Many, if not most, other empirical studies face the same problem of not observing the relevant risk measure over time, as pointed out by Hwang *et al.*: "While studies of this sort [i.e. panel studies] represent improvements over standard cross–sectional studies, their applicability is restricted by the availability of longitudinal data sets that include the relevant nonwage job attribute variables. In most cases, this is a binding constraint." (Hwang *et al.*, 1992, p. 836).

wages ceteris paribus. To stick with the model from equation (4.1), the hedonic wage regression with unobserved individual heterogeneity made explicit can be written as:

$$y_{ijk} = \alpha + x_i'\beta + z_j'\gamma + \delta r_k + \theta_i + \epsilon_{ijk} \tag{4.3}$$

where $(\theta_i + \epsilon_{ijk})$ corresponds to the error term u_{ijk} in equation (4.1) whereby now we make the problem of individual heterogeneity explicit (for simplicity, θ is rescaled such that the partial of effect of θ on y is equal to 1).[7] Now, even if we can assume that ϵ_{ijk} *is* mean independent of (x, z, r), identification of the compensating wage differential δ is only achieved if the unobserved effect θ is also mean independent of (x, z, r). Whenever there is reason to believe otherwise, parameter δ is not identified (and neither are the other parameters identified, but that is of minor importance for our purposes, since we are not per se interested in these parameters).

As discussed in section 4.2, the leading reason for a correlation between θ and the accident risk r is that θ reflects unobserved productivity, which is obviously related to the wage y. If the demand for safety actually increases with income and if we are, at the same time, unable to adequately control for productivity differences, then this could quite plausibly lead to a correlation between θ and r. That is, more productive workers (i.e. workers with above–average θ) sort themselves into less–risky jobs by accepting lower wages, which in turn leads to a correlation between the productivity measure θ and the risk measure r, meaning that identification of the risk compensation parameter δ must ultimately fail.

In the following, we will discuss three different empirical approaches in turn, all of which are intended to mitigate the worker–sorting leading to biased estimates of the compensation for risk.

4.4.2 Control Function

The first approach, which we might label control–function approach, is to basically stick with the hedonic wage regression, but to try to control for as many observable characteristics (both at the individual and the firm/job level) as possible. In fact, controlling for the appropriate set of observed variables might entail identification of δ, depending on which variables are observed, and thus can be controlled for in the regression model. Under 'typical' circumstances however, this approach is prone to fail identification, since the data sources usually available do not include enough control variables or the critical control variables, respectively. Nonetheless, we will also estimate hedonic wage regres-

[7] Note that the error term ϵ_{ijk} potentially also includes unobserved heterogeneity with respect to the firm. We will take up this issue in section 4.4.4 below.

sions, mainly for reasons of comparison. We stress here that we would not place much confidence in the resulting estimates for the parameter δ. The bottom line is that this approach to identification crucially hinges on the availability of enough control variables (describing both the workers and the jobs).

4.4.3 Sample Stratification

A second related approach is to stratify the sample in such a way as to minimize the variation in the unobserved error component θ (see equation (4.3)). That is, we run the very same hedonic wage regression as given by equation (4.1), but only on a narrow subset of individuals. Ideally, this subset consists of individuals presumably as similar as possible with respect to θ. That is, stratification is the simple non–parametric counterpart of the control function approach. However, since most often it is very difficult to control for θ, we think that stratifying the sample is probably a more fruitful approach.

Our stratification variable of primary interest is the skill–level of the job, which is recorded in the SWSS. Let $s_{ij} \in \{1,2,3,4\}$ be the skill–level of individual i working in job j, where $s = 1$ ($s = 4$) corresponds to the highest (lowest) skill–level of a given job. We thus run the same hedonic wage regression as in equation (4.1), but only on a subset of individuals within a given skill–level s. Specifically, we will run the following regressions:

$$y_{ijk} = \alpha + x_i'\beta + z_j'\gamma + \delta r_k + u_{ijk} \quad s_{ij} \geq s \in \{1,2,3,4\} \tag{4.4}$$

Note that this approach to estimation is basically the same as the control function approach, the main difference being that stratification allows *all* parameter estimates to vary between different subsets of the sample[8]. However, we think it plausible that the main advantage of the stratification is that we can minimize variation in θ in this way, which ideally renders a consistent estimate of the compensating wage differential δ.[9]

[8]That is, the control function approach yields the same estimates as sample stratification if all parameters would be interacted with the variable on which stratification is based on. However, such a fully interacted regression model is, due to the large number of parameters to be estimated, often difficult to interpret.

[9]As we will show later, our stratification approach actually reduces the differences between groups of workers with respect to the observed wage (on this point, see table 4.5). For example, in the overall sample the difference in mean monthly earnings between men and women amounts to about 1,700 Swiss francs (about one third relative to the female average). In the subsample of workers within the lowest skill–level, the difference in average earnings amounts to only about 630 Swiss francs (relative to the female average, a bit less than 15%). Although this is only suggestive evidence, we still believe that this exactly what one would expect if the presumption holds that the variance in θ is lower in the lower skill–levels of jobs.

4.4.4 Wage Decomposition and Firm Wage–Component

Our third approach to identification and estimation is based on quite another idea, which tries to capitalize on the availability of panel data (with respect to the firm).[10] Still, we can use the additional source of variation in wages stemming from the fact that the SWSS has a longitudinal structure (at least with respect to the firm) such that we can apply simple panel data methods (see, for example, Wooldridge, 2002).

To start with, let us assume that the observed natural logarithm of the wage y_{it} of individual i in a given year t can (conceptually) be decomposed in a linear model as follows:

$$y_{ijt} = \lambda_t + \phi_i + \psi_j + \epsilon_{ijt} \tag{4.5}$$

Abstracting from the time fixed–effect λ_t, equation (4.5) states that individual i's wage is the sum of an individual wage fixed–effect ϕ_i, a firm wage fixed–effect ψ_j, and a remaining random error component ϵ_{ijt}. The critical assumptions in this simple linear fixed effects model are the assumptions about the time invariance of both the individual and the firm fixed effect. However, since we are using panel data spanning only a short time period we believe that these assumptions are innocuous for our application – nonetheless allowing us to resort to the power of panel data methods. Importantly, note that the theory of compensating wage differentials essentially makes statements about the wage component specific to the employer (i.e. ψ_j), but not to the individual–specific part nor the random part of the wage.

This simple representation of the wage essentially states that the wage of a specific individual i in a given year t is the sum of an aggregate time effect (e.g. aggregate shocks), an individual–specific component (which is assumed to be time–invariant), a firm–specific part (also assumed to be time–invariant) and a random error term (varying over time, firms, and individuals). If it is possible to consistently estimate the wage firm fixed effect ψ_j from the available data, we can essentially get rid of individual heterogeneity by simply running a hedonic wage regressing using the estimated wage firm–fixed effect $\hat{\psi}_j$ instead of the observed wage y_{ijt} on our risk measure r, although we can not directly control for unobserved individual heterogeneity in the hedonic wage regression (because, remember, the risk measure is *not* observed over time and because there is no person–identifier in the SWSS).

Thus, in a first stage, we run a simple regression model using the three consecutive

[10]Of course, we could capitalize on repeated individual observations using for example the techniques proposed by Abowd and Kramarz (1999), but as explained in section 4.3, we only have temporal information about the employer but not the individual workers.

waves of the SWSS:

$$y_{ijt} = \alpha + x'_{it}\beta + z'_{jt}\gamma + \lambda_t + \psi_j + u_{ijt} \quad \text{with} \quad s_{ij} = 4 \tag{4.6}$$

Here, again, x and z are vectors of observed individual and firm characteristics and the parameter λ_t captures aggregate wage shifts over time. The vector x of observed individual characteristics is important here because we essentially use x to proxy for otherwise unobserved individual heterogeneity. Moreover, we run this regression on a subset of individuals working in jobs with the lowest skill–level only, such that we can further dampen the problem of unobserved heterogeneity.

The regression model given by equation (4.6) is only of interest here because it allows us to estimate the firm wage fixed effects, represented by the vector ψ_j. Practically, ψ_j is estimated from the data by including a separate dummy variable for each firm in the sample.

In the second stage, we run a regression very similar to the hedonic model from equation (4.1):

$$\hat{\psi}_{ijk} = \alpha + x'_i\beta + z'_j\gamma + r_k\delta + u_{ijk} \quad \text{with} \quad s_{ij} = 4 \tag{4.7}$$

where now the dependent variable is the estimated firm wage fixed effect $\hat{\psi}_{ijk}$ of individual i working in firm j. Note that the unit of observation is still the individual worker, although the firm fixed effect obviously does not vary between individuals working in the same firm. This procedure, though, directly applies the right weighting scheme. Again, r_k is the non–fatal risk measure in industry×skill–level cell k. Note that we still have to include both vector x and z, because the estimated wage firm fixed effect $\hat{\psi}$ is not independent of x and z. The main point is that the estimated wage firm fixed effect $\hat{\psi}$ should have been separated from the unobserved individual–specific component θ.

4.5 Econometric Results

We now present the econometric results, starting with some simple hedonic wage regressions. We then go to discuss the results from stratifying the sample by skill–level, which yields results in the expected direction. Next, we present results from our preferred approach, regressing firm wage fixed–effects instead of individual wage on accident risk. Finally, we present empirical estimates for the statistical value of a injury (i.e. a non–fatal accident related to workplace activities), which are implicitly given by the estimates of the different econometric models.

4.5.1 Hedonic Wage Regression

Estimated parameters of the hedonic wage function, as given by equation (4.1), are given in table 4.2 (column 1). The point estimate of the non–fatal accident risk is negative (-0.00005), although statistically not different from zero (t–value of about less than one in absolute value). This result is in fact in line with either endogenous sorting of workers. Note also that the other regressors have the expected sign. As discussed in section 4.4, the leading explanation for the "wrong" sign of the risk variable is endogenous sorting of workers into jobs with different risks. As we do not put much confidence in this simple hedonic wage regression, so we quickly move on to the next results.

4.5.2 Sample Stratification

Columns 2 to 4 in table 4.2 also show parameter estimates from a simple hedonic wage regression, but only for a subset of workers each. As we narrow the range of the skill–level, the point estimate of risk compensation moves towards the expected direction. Focusing on workers in the lowest skill–level only yields a positive point estimate on the risk measure (0.00024), which moreover is almost statistically significant on the 10% level (t–value of 1.63). The decrease in the R–squared of the model reflects the fact that the stratification of the sample absorbs a large part of the variation in the regressors (e.g. educational attainment; see section 4.3), which otherwise explain a significant part of the variation in wages.

4.5.3 Wage Firm Fixed Effects

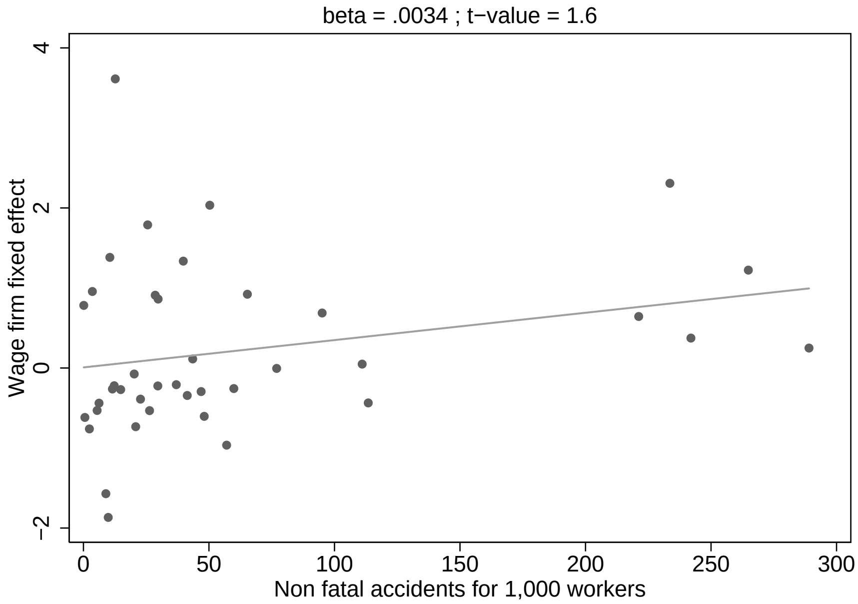
The last column in table A.1 in the chapter appendix shows the estimated firm wage fixed–effect by industry (at the two–digit level, only for the lowest skill–level of jobs). As shown in figure 4.2, a simple scatterplot of the average firm wage fixed–effect (averaged within industries) versus the number of non–fatal accidents now shows a clear positive relation between the two variables (as opposed to figure 4.1, which showed no relation between the two measures at all). A simple regression of the average wage firm fixed effect on the number of non–fatal accidents yields an estimated slope coefficient of 0.0034, which marginally reaches statistical significance (t–value of about 1.6). Column 1 of table 4.3 reproduces, for the purpose of comparison, the simple hedonic wage regression using workers from the lowest skill-level only (see section 4.5.1 above). As it turns out (see column 2, table 4.3), the point estimate of the risk parameter more then doubles when using $\hat{\psi}$ instead of y directly as the dependent variable in the regression, yielding a point estimate of 0.00067 (with a t–value of more than 2). This result is in line with the story of workers sorting into jobs based on their (partially) unobserved productivity,

Table 4.2: Hedonic wage regressions, by skill–level of job

	ln(monthly wage) (y)			
Skill–level(s) of job	1–4	2–4	3–4	4
Non fatal accident risk	−0.00005	−0.00003	0.00001	0.00024
	(−0.88)	(−0.60)	(0.30)	(1.63)
(Plant size / 100)	0.00193**	0.00179**	0.00175**	0.00148*
	(2.92)	(2.98)	(2.77)	(2.16)
(Plant size / 100) squared	−0.00001**	−0.00001***	−0.00001**	−0.00000*
	(−3.49)	(−3.60)	(−3.31)	(−2.12)
Age	0.03299***	0.03329***	0.03028***	0.01733***
	(17.36)	(17.10)	(15.80)	(9.61)
Age squared	−0.00034***	−0.00035***	−0.00032***	−0.00019***
	(−18.23)	(−17.70)	(−16.27)	(−9.18)
(Tenure / 10)	0.07161***	0.07189***	0.07910***	0.11261***
	(5.06)	(5.13)	(5.63)	(6.65)
(Tenure / 10) squared	−0.00834*	−0.00807*	−0.00873*	−0.01767***
	(−2.40)	(−2.37)	(−2.64)	(−4.19)
Constant	8.91118***	8.70657***	8.59138***	8.67286***
	(121.62)	(119.71)	(120.24)	(50.83)
n	468,328	441,269	346,916	130,976
R^2	0.623	0.556	0.459	0.321

Notes: *, **, *** denotes statistical significance on the 5%, 1% and 0.1% level, respectively. Robust t–values in parentheses. Skill–level 1 (4) corresponds to the highest (lowest) skill–level possible.

Figure 4.2: Firm fixed effect versus non–fatal injury risk, by industry



Notes: The y–axis shows the average of the wage firm fixed effect and the x–axis the number of non–fatal accidents per 1,000 workers per year. Workers in lowest job skill–level only. Also see table A.1 in the chapter appendix. Own calculations, based on SWSS (2004) and SAIF (2004).

because the main difference between columns 1 and 2 of table 4.3 is that variation in y still reflects to a large part variation in unobserved productivity, whereas variation in $\hat{\psi}$ much less so.

4.5.4 Detailed Results

We present some additional results for different subgroups of the sample, based on both the simple hedonic wage model and on models using the wage firm fixed effect as the dependent variable. The estimates of these additional models are given in table 4.4. These additional estimates are consistent with our main result, since in each case the model using the wage firm fixed effect as the dependent variable yields a higher risk compensation than using the observed wage. Panel A of table 4.4 simply reproduces the result from table 4.3 discussed above for easy comparison with the other results.

Additionally, these estimates may shed some light on the question of the sorting of workers into firms with different risk compensation and possibly on differences in risk aversion between groups of workers.[11] Note that, by construction, the estimated firm wage fixed effect $\hat{\psi}_{ijk}$ is the same for all individuals working within a specific firm j.

[11] We also split the sample by marital status (i.e. married versus single individuals). We did not find (statistically) different results and we thus do not present these results.

Table 4.3: Observed wage versus wage firm fixed effect (skill–level 4 only)

	ln(monthly wage)	
	Observed wage (y)	Firm fixed effect ($\hat{\psi}$)
Non fatal accident risk	0.00024	0.00067⋆
	(1.63)	(2.41)
(Plant size / 100)	0.00148⋆	0.00189⋆
	(2.16)	(2.10)
(Plant size / 100) squared	−0.00000⋆	−0.00000
	(−2.12)	(−1.63)
Age	0.01733⋆⋆⋆	0.00437⋆
	(9.61)	(2.49)
Age squared	−0.00019⋆⋆⋆	−0.00005⋆
	(−9.18)	(−2.68)
(Tenure / 10)	0.11261⋆⋆⋆	0.03799⋆⋆
	(6.65)	(2.80)
(Tenure / 10) squared	−0.01767⋆⋆⋆	−0.00793⋆
	(−4.19)	(−2.42)
Constant	8.67286⋆⋆⋆	−0.08190
	(50.83)	(−1.31)
n	130,976	130,976
R^2	0.321	0.201

Notes: ⋆, ⋆⋆, ⋆⋆⋆ denotes statistical significance on the 5%, 1% and 0.1% level, respectively. Robust t–values in parentheses. Only workers in jobs of lowest skill–level. Own calculations, based on SWSS (2004) and SAIF (2004).

It thus must be the case that differences in the estimated risk compensation between subgroups of workers somehow reflect differences in risk compensation between firms. We will be more explicit on this point below when discussing the results.

First, we split the sample by gender (panel B of table 4.4). The hedonic wage model gives positive point estimates for both men and women, although both are not statistically different from zero. Interestingly, the point estimate of the compensating wage differential is larger for women ($\hat{\delta} = 0.00046$) than for men ($\hat{\delta} = 0.00015$). Using the wage firm fixed effect yields, in both cases, a higher point estimate than using the observed wage ($\hat{\delta} = 0.00038$ for men, and $\hat{\delta} = 0.0015$ for women), but now in this case both coefficients are statistically different from zero. Still, the estimate for women remains about three times as large as the corresponding estimate for men.

We believe that such a pattern is informative with respect to the underlying sorting of workers into firms with different risk compensation. The results essentially state that women ask a higher risk compensation than men *for a given* change in the statistical non–fatal accident risk. This result is in line with empirical evidence on differences in risk aversion between men and women (Sunden and Surette, 1998).

Table 4.4: Wage firm fixed effects, detailed results

	Log–Wage	Firm fixed effect
A. Overall sample	0.00024	0.00067*
	(1.63)	(2.41)
n	130,976	130,976
R^2	0.321	0.201
B. By gender		
Men	0.00015	0.00038***
	(0.96)	(3.81)
n	60,219	60,219
R^2	0.304	0.192
Women	0.00046	0.00150*
	(1.34)	(2.61)
n	70,757	70,757
R^2	0.240	0.270
C. By size of firm		
Smaller firms	0.00031**	0.00070**
	(3.29)	(3.36)
n	75,911	75,911
R^2	0.293	0.185
Larger firms	−0.00001	0.00055
	(−0.02)	(1.09)
n	55,065	55,065
R^2	0.395	0.243

Notes: *, **, *** denotes statistical significance on the 5%, 1% and 0.1% level, respectively. Robust t–values in parentheses. Own calculations, based on SWSS (2004) and SAIF (2004).

Second, table 4.4 (panel C) also shows separate results for smaller (that is, less than 500 employees) and larger (500 or more employees) firms. The simple hedonic wage regression for smaller firms gives us a positive and significant point estimate for risk compensation ($\hat{\delta}$=0.00031, t–value of about 3.3). For larger firms, we find no effect of accident risk on the firm wage fixed effect (the point estimate is even negative). Moving on to the fixed effects regression, we again get a larger point estimate for the smaller firms ($\hat{\delta}$ = 0.0007, t–value of about 3.4) and larger firms ($\hat{\delta}$ = 0.00055), although for larger firms the estimate remains statistically insignificant.

This result states that smaller firms have to pay higher risk compensation for any increase in the risk of non–fatal accident than larger firms do. This difference in risk compensation might reflect underlying differences in the wage setting process between firms of different size. Specifically, one might argue that wages in smaller firms are more likely to reflect competitive wages than in larger firms, where rent sharing is presumably more prevalent than in smaller firms. Another possible explanation for this finding is that workers may perceive working at larger firms per se as more safe (for whatever reason). In statistical terms, in fact, larger firms do not pay any risk compensation at all, which possibly means that larger firms have to guarantee workplace safety anyway because they are presumably under stricter monitoring, whereas smaller firms have more discretion with respect to workplace safety and thus to risk compensation.

4.5.5 The Value of a Statistical Injury

Given an estimate for the compensation for non–fatal accident risk, we can easily compute the value of a statistical injury (i.e. non–fatal accident). Because all our estimates of the risk parameter are based on semi–logarithmic regressions, the estimated risk coefficient corresponds to the *relative* wage which 1,000 workers are willing to forego in order to prevent one non–fatal accident (and thus is independent of the time period chosen). Thus, multiplying the estimated risk parameter by 1,000 yields the estimated *relative* value of a statistical injury (VSI):

$$\text{VSI} = \hat{\delta} \cdot 1,000 \tag{4.8}$$

Since our risk measure refers to non–fatal accident per year, we will phrase the VSI in terms of average annual earnings (that is, we multiply VSI additionally with the average annual earnings in the corresponding group of workers). Table 4.5 shows estimates for the value of a statistical injury computed from the different estimation methods discussed above (given in terms of the average annual earnings in the sample). The main estimates are based on the point estimate of the risk variable. Lower and upper bounds on the value of a statistical injury are based on the 95% confidence interval of each point estimate of the parameter δ. The simple hedonic wage regression actually yields a negative estimate for the value of a statistical injury (per injury per year). Only using the upper bound of the confidence interval yields the expected positive value (although still small).

Stratification of the sample yields a higher value of a statistical injury, the narrower the sample. Focusing on workers in the lowest skill–level only gives an estimate of about 14,000 Swiss francs (the estimate based on the lower bound of the confidence interval though still gives a negative estimate).

Using the wage firm fixed effect finally gives a consistent positive value of a statistical injury (even if we use the lower bound of the corresponding confidence interval). Using the point estimate, we get an estimated value of a statistical injury of about 40,000 Swiss

Table 4.5: The estimated value of a statistical injury

	Yearly earnings	Estimated value of a statistical injury (VSI), based on		
		Lower bound of $\hat{\delta}$	Point estimate of $\hat{\delta}$	Upper bound of $\hat{\delta}$
A. Hedonic wage function				
Skill–level 1–4	76,464	−12,512	−3,823	4,866
B. Stratification				
Skill–level 2–4	71,496	−9,294	−2,145	5,005
Skill–level 3–4	64,572	−3,659	646	4,951
Skill–level 4 only	54,324	−2,959	13,038	29,035
Men	58,380	−9,487	8,757	27,001
Women	50,856	−11,522	23,394	58,310
Smaller firms	54,120	6,578	16,777	26,976
Larger firms	54,588	−55,134	−546	54,042
C1. Wage firm fixed effect				
Skill–level 4 only	54,324	6,192	36,397	66,602
C2. Wage firm fixed effect: Subsamples				
Men	58,380	10,539	22,184	33,830
Women	50,856	17,829	76,284	134,739
Smaller firms	54,120	15,334	37,884	60,434
Larger firms	54,588	−25,065	30,023	85,112

Notes: All entries are based on the point estimate, the lower and upper bound of the 95% confidence interval of $\hat{\delta}$, respectively. Own calculations, based on SWSS and SIAF.

francs per non–fatal accident averted per year. This value fits into the range reported by most other studies (see Viscusi and Aldy, 2003, again).

4.6 Conclusions

We provide empirical estimates of the value of a statistical injury for Switzerland for the year 2004, using non–fatal accident risk within industry×skill–level cells and applying different approaches to identification. Specifically, we try to statistically isolate the firm–specific wage component, to which the theory of compensating wage differentials conceptually applies most directly. Further, we try to mitigate the problem of endogenous worker sorting as far as possible by combining appropriate data and methods.

The empirical method actually makes a huge difference with respect to the estimation of risk compensation. Simple hedonic wage regressions actually yield negative or zero compensation for non–fatal accident risk at the workplace. Moving on to methods we believe are more reliable (i.e. consistent) pushes the risk compensation in the ”right” direction (i.e. yielding positive compensation for accident risk). Our preferred estimation method, based on a restricted sample of workers in jobs of lowest skill–level only and using the wage firm fixed effect instead of the observed wage, gives an estimate for the value of a statistical injury of about 40,000 Swiss francs, which is within the range given by both studies from inside and outside the U.S. labor market.

Our analysis, by comparing the magnitude of risk compensation, may also shed some light on the problem of endogenous sorting of workers based on their (unobserved) productivity–relevant characteristics. The more attention we pay to mitigating unobserved productivity differences, the larger the estimates for risk compensation we get. This pattern seems to be consistent with the hypothesis that high–productivity workers select into lower–risk jobs by accepting lower wages.

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4.A Appendix

Table A.1: Main variables, by industry (lowest skill–level only)

Industry	Workers	Earnings	Accidents	FFE
Petroleum refining and processing	4692	5,560.13	0.14	0.78
Office material production, data processing	4288	4,302.83	0.59	-0.62
Information technology services	6237	3,933.30	2.39	-0.76
Shipping	55	5,467.47	3.57	0.96
Metal production and processing	7201	4,781.81	5.46	-0.53
Aviation	12	5,496.25	6.22	-0.44
Production of leather goods and shoes	229	3,628.01	8.94	-1.57
Production of clothes and fur goods	270	3,741.27	9.89	-1.87
Insurance industry	2086	5,300.57	10.53	1.38
Production of medical technology	7421	4,523.07	11.55	-0.26
Retail business	19118	4,090.10	12.26	-0.22
Tobacco processing	636	5,977.87	12.70	3.61
Production of furniture, jewellery, musical intruments	1743	4,329.91	14.86	-0.27
Machinery/mechanical engineering	5441	4,851.64	20.24	-0.07
Textiles	1350	4,436.00	20.83	-0.73
Automobile industry	1075	4,508.15	22.73	-0.39
Energy– and watersupply	496	5,504.46	25.59	1.79
Traffic support	1502	4,360.78	26.35	-0.53
Credit business	3059	5,833.48	28.60	0.91
Paper and carton production	2153	4,917.06	29.64	-0.22
Credit business and insurance industry	70	5,373.94	29.76	0.86
Printing, publishing and distribution industries	3013	4,833.14	36.99	-0.21
Research and development	202	5,478.94	39.78	1.34
Whole sale	7621	4,683.02	41.36	-0.34
Wood processing	810	4,950.09	43.53	0.11
Transportation	2236	4,724.08	46.89	-0.29
Rubber and plastic production	2657	4,511.65	48.12	-0.60
Mining	80	5,277.08	50.33	2.03
Agriculture	6756	4,310.73	57.05	-0.96
Mining	1217	4,821.76	59.91	-0.26
Health and welfare system	19642	4,582.02	65.31	0.92
Hotel and restaurant industry	9676	3,743.90	76.98	-0.01
Real estate	581	4,784.07	95.10	0.69
Information transmission	55	4,707.71	111.04	0.05
Entertainment	814	4,208.07	113.46	-0.44
Education	744	4,394.47	221.19	0.64
Personal services	238	4,318.43	233.62	2.31
Waste management	95	4,953.19	242.00	0.37
Lobby, associations, organizations	512	5,067.35	264.88	1.22
Construction	4893	4,965.64	289.03	0.25

Notes: Table entries show sample averages within industries. Non–fatal accident risk is the number of non–fatal accidents per 1,000 workers. Wage is the average logarithm of gross monthly earnings. Wage firm fixed effect is the average firm fixed effect, as given by equation (4.6), and is (in the table) standardized to mean 0 and variance 1. Own calculations, based on SWSS and SIAF.

CHAPTER

FIVE

CLOSING WORDS: SEEING THROUGH STATISTICS

> ”Statistics are like bikinis.
> What they reveal is suggestive,
> but what they conceal is vital.”
>
> Aaron Levenstein (1911–1986), business economist

”Seeing through statistics” (which is the title of a wonderful textbook on statistics (Utts, 2004)) actually is often not that easy, but still I think that empirical methods, combined with suitable data, can shed light on practically any research topic. In a nutshell, the three main lessons I learned while working on my own projects are the following. First, good empirical research is a combination of good data, a clever design and appropriate econometric methods. Second, more often than not, some ambiguity will remain at the end of the day. And third, one has to prepare for the unexpected and should not let oneself be misdirected by one’s own preconceptions.

For example, chapter 2 has shown that beside the data the design of a study is of crucial importance for causal inference. However, I also think that both ingredients are about equally important. That is, sophisticated methods will never rescue garbage data (”garbage in, garbage out”, as the proverb goes). Fortunately, I’m convinced that all three essays presented in this thesis rely on high–quality data although, of course, all the data I use suffer from some shortcomings. In the end, you will always have to compromise when doing empirical research either way (Hamermesh, 2000). For example, in chapter 4, although we have access to more detailed data than most previous studies we still would have liked even more detailed data (i.e. ideally we would have used wage data with a longitudinal structure not only with respect to the employer, but also to the individual workers). This, of course, is also true regarding the data used in chapter 3. These data exemplify both the strengths and weaknesses of typical survey data. On the one hand, these data tend to be very rich in that they provide many different variables that normally are not available in administrative data sources. On the other hand, such data in most cases suffer to some extent from measurement problems and sampling problems (due to missing data). Still, I think, they give empirical researchers the opportunity to explore fascinating questions which administrative (or experimental) data would otherwise not allow.

On the other hand, even if the data seem to fit (more or less) perfectly your question, you still need appropriate econometric methods to handle the data. That's especially true for the results presented in chapter 2, which clearly shows that "simple" methods in this case fail to account for the problem of reverse causality. In some cases then, simple methods give presumably not the right answer to your question. But, because there is no "free lunch" in statistics, you need access to more information (i.e. more data) than is usually available in typical data sets in order to use more powerful techniques.

Another simple fact of empirical research is, and that's a point everyone knows who has ever done empirical analysis, that seemingly simple questions often turn out not to be that simple. That's a lesson I especially learned while working on chapter 2. We started with the premise that unemployment (at least if not involuntarily chosen) is "bad" for most people. And, at first sight, the data indeed suggested this premise to be true, as the data showed a huge increase in overall health costs after the date at which workers lost their job (relative to workers not losing their jobs). However, as we dug deeper, we found that a significant part of this increase in health costs was presumably not driven by deteriorating health per se, but rather by a "mechanical" effect, in that many workers seem to go on sick leave before entering registered unemployment. This effect in turn most likely is due to the fact that sick leave payments are higher than unemployment benefits (although this interpretation is also not as straightforward as it seems, but still seems plausible). On the other hand, the interpretation of the results itself must also often remain somewhat ambiguous in the end, simply because the data do not speak for themselves and the results need to be interpreted. The interpretation of empirical results, however, is most often open to some debate in the end, because we never know how the data were "generated" in the first place. And, it is of course also true that empirical research itself is often more "art" than "science", since you have to compromise a lot because real–world data never perfectly fit any one econometric model. Still and finally, I am convinced that empirical research has a lot to offer with respect to our pursuit of understanding how the social world is working.

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